The Engineering Review.

Edited by

C. EDGAR ALLEN, A.M.I.E.E., Etc.

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THE INTERNAL COMBUSTION ENGINE.

THE position attained by the gas engine to-day justifies the amount of attention that was directed to problems connected therewith at the recent Dublin meeting of the British Association. Although Mr. Dugald Clark, in addressing the engineering section, dealt historically with the subject of thermodynamics, the point of view adopted by him had reference more particularly to the internal combustion engine.

Again, the report of the British Association committee on gaseous explosions is chiefly of interest because of its bearing upon the theory of prime movers of this class, and presents a valuable analysis of the present state of knowledge on this subject together with suggestions as to the directions in which further research may be undertaken with the object of advancing it.

Our understanding of the many points involved in connection with the gas engine has to-day reached a satisfactory stage, and although much difference of opinion still exists on physical and chemical points of the highest scientific interest, yet practice has reached a certain definite stage, in so far as main features are concerned. These may be grouped as follows:– Initial compression of the charge, exposure of the smallest area of cooling surface to the flame for a minimum time, and the use of lowest flame temperatures consistent with high available pressures.

For the purposes, however, of formulating an accurate standard of comparison in estimating the thermodynamic efficiency of the gas engine, much still requires to be known concerning the physical and chemical properties of the gases involved in the process of combustion or explosion, particularly at the temperatures occurring in the gas engine, that is, between 1,000° and 2,500° C., and it is these matters that have been considered proper subjects for investigation by the committee.

Already heat efficiencies have improved from 15 per cent. to 30 per cent., but future practice and further improvements — which cannot be doubted — must be more or less intimately connected with the progress of knowledge on these points.

In the meantime the popularity of the internal combustion engine is assured, and its keen rivalry to steam in land application is, in the not too distant future, likely to be as vigorous in marine service. Since the advocacy of Mr. James McKechnie, of Messrs. Vickers, Sons & Maxim, of marine gas-engine propulsion on a large scale, much attention has been directed to the subject, and with an adequate realisation of the nature of the difficulties involved, such as will undoubtedly be obtained by the systematic procedure now being adopted by the Admiralty, the new era now foreshadowed in ship propulsion should not be far distant.

XUM

A New Change-Speed Gear.

Particulars have reached us of an ingenious attempt to solve the problem of the change-speed gear by avoiding the use of toothed wheels, and to provide a greater range of speed and more gradual acceleration than is possible with gearing.

The novelty consists in the method of obtaining the drive, which is through a series of rotary impulses, alterations in the speed being effected by varying the number of such impulses. This method provides for a uniform rotation even on the lowest speed of one impulse per revolution, a fact which may be better understood when it is remembered that with a petrol engine running under normal conditions only from 10 to 20 impulses per second are given.

Details of the gear are shown in Fig. 1, but the main features of its operation will be better understood from Fig. 2.

This gear is designed to give an out-of-gear position, three forward speeds and one reverse speed, and is therefore specially adapted for use on motor cars.

Fig. 2 shows the gear in the off position, and the forward speeds are obtained through the six hardened steel rollers R^1 to R^6, which are made to engage with the inside circumference of a cast-steel lining B^1, mounted inside a wheel keyed to the driven shaft. The rollers are carried in the carriers D^1 to D^6, pivoted in the driving wheel A^1, which is keyed to the primary shaft A. The floating cylinder B^1 is fitted with a pneumatic tube, and is so mounted on the secondary driving wheel B^2 to allow of slight radial movement, but keyed to prevent independent rotation; the driving wheel B^2 being keyed to the driven shaft B. No power whatever is transmitted through the pneumatic tube shown on the drawing, its object being to facilitate to the rollers an easy entrance to, and exit from, engagement, consequently reducing to a minimum the noise and wear.

The primary shaft is eccentric to the driven shaft, so that each roller when in gear only engages with the floating cylinder through an angle of about 60 deg., and the variation of speed is obtained by varying the number of rollers in gear. The rollers are brought into action by the cam wheel A^2, which when rotated lifts the carriers. The running positions are obtained when each pair of opposite rollers have been brought into action, that is, rollers R^1 and R^2 give first speed; R^1, R^2, R^3, and R^4 the second; and R^1, R^2, R^3, R^4, R^5, and R^6 the third. The rollers R^1, R^3, and R^5 come into action slightly in advance of R^2, R^4, and R^6 respectively, so as to accelerate gradually.

The independent rotary movement of the cam wheel is obtained through the operating collar C, which keys the cam wheel to the shaft A in such a manner that if the collar is moved longitudinally, it causes the cam wheel to lag behind or

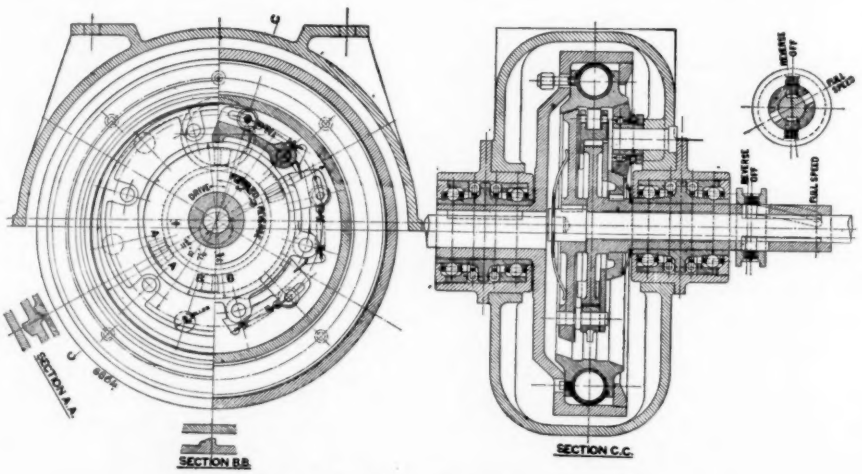

FIG. 1.—DETAILS OF THE WOODFIELD-HEWER CHANGE-SPEED GEAR.

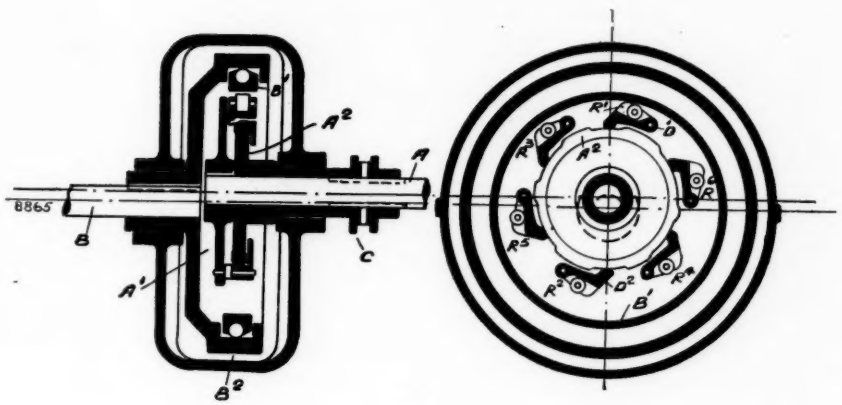

FIG. 2.

advance in front of the shaft A and the driving wheel A^1.

The final drive is obtained by pressing a friction wheel into direct and continuous contact with the steel lining, the driving rollers being put out of action at the same time.

The reverse drive is obtained by the same sliding movement in the opposite direction.

The two shafts are mounted in ball bearings, which are fixed to the top half of the case, the bottom half of which can, therefore, be taken off for inspection purposes.

One of the main advantages claimed on behalf of the gear is that in changing from one speed to another no retardation can take place. A comparison of the acceleration curves shown in Fig. 3 of the various speeds of a spur gear shows that the second and third speeds commence to accelerate some distance down on the curve of the preceding speed, whereas with the gear the curve from one speed continues on to the one succeeding. It is also obvious from the drawing that the preceding gears remain in when those succeeding are put in.

From tests made on a 20 h.p. gas engine, we are informed that, on any particular notch there was practically no variation in speed from half load to full load, and on any notch it was possible to pull up the gas engine by applying the brake through the gear. On the above test a variation of $3\frac{1}{2}$ to 1 was obtained, and although the gear under test was very crudely made, the results were satisfactory.

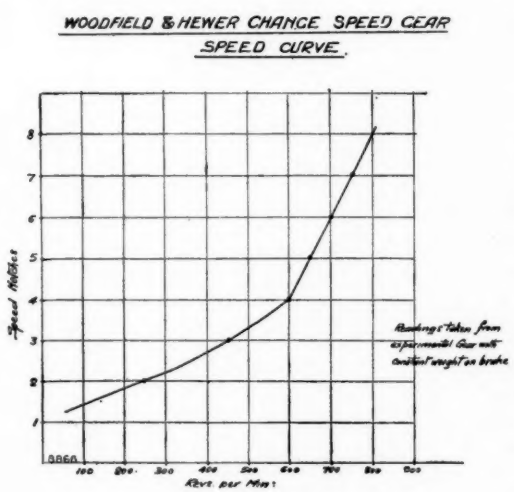
WOODFIELD & HEWER CHANGE SPEED GEAR
SPEED CURVE.
Speed Ratios
Revs. per Min.
Readings taken from experimental Gear with constant weight on brake
8866

FIG. 3.

The Design of Semi-Elliptic Springs.

By RALPH WOLFENDEN, M.Sc.

SEMI-ELLIPTIC springs, as usually made, contain several full-length leaves and several leaves of varying lengths.

The two sides of the springs may be treated as cantilevers of lengths equal to $\frac{L}{2} - \frac{w}{2}$ where L is the length from tip to tip and w is the width of the band.

The first set of leaves, of constant length, form a cantilever of constant cross-section, and the second set, of varying lengths, form a cantilever of uniform strength.

The object of this paper is to inquire into the correct proportioning of these varying lengths, having given the width of the band at the centre, the length of the spring, and the load to be carried at the centre of the spring.

Let $2W$ be the central load.

Then a load of W acts on each end of the spring.

$$\text{Let } l = \frac{L}{2} - \frac{w}{2}.$$

Numbering the successive leaves by 1, 2, 3, 4, 5, 6, 7, &c., then in Fig. 2 Nos. 1, 2 and 3 form a cantilever of uniform cross-section, and the remaining leaves 4, 5, 6, 7 and 8 form a cantilever of uniform strength.

Let W_1 be portion of load carried by cantilever of uniform strength, and W_2 be portion carried by cantilever of uniform cross-section.

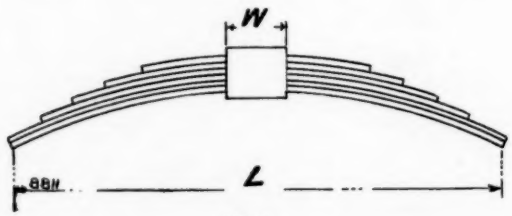

FIG. 1.

$$W_1 + W_2 = W.$$

For cantilever of uniform cross-section, take origin at fixed ends.

$$EI_2 \frac{d^2y}{dx^2} = W_2 (l - x)$$

Where I_2 = moment of inertia of section of this cantilever.

$$EI_2 \frac{dy}{dx} = W_2 lx - \frac{W_2 x^2}{2}$$

$$EI_2 y = \frac{W_2 l x^2}{2} - \frac{W_2 x^3}{6}$$

Maximum deflection occurs when $x = l$.

$$\text{Then } \delta_2 = \frac{W_2 l^3}{3EI_2}$$

if δ_2 is deflection of this portion of spring.

Let f_2 be stress at skin.

$$\text{Then } \frac{W_2 l}{I_2} = \frac{\frac{f_2}{t}}{2} \therefore f_2 = \frac{W_2 lt}{2I_2}$$

where t = thickness of each of the leaves.

Let n_2 be number of leaves in this cantilever,

and b = breadth of spring.

$$\text{Then } I_2 = \frac{n_2 bt^3}{12}$$

$$\text{Hence } f_2 = \frac{12\ W_2 lt}{n_2 bt^3 \times 2}$$

$$= \frac{6W_2 l}{n_2 bt^2}$$

For cantilever of uniform strength.

In a beam of uniform strength we have

$$\frac{M}{I} = \frac{f}{y} = \frac{f_o}{y_o}$$

where f_o = skin stress and y_o is distance of this stress from neutral axis.

Now $\frac{f_o}{y_o}$ is constant throughout a beam of uniform strength.

$\left(y_o = \frac{t}{2} \text{ in our case}\right) \therefore \frac{M_o}{I_o} = \frac{f_o}{y_o}$ = constant.

Where M_o and I_o are the bending moment and moment of inertia of section at end of cantilever.

If n_1 = number of leaves in this cantilever,

Then $I_o = \frac{n_1 bt^3}{12}$

Since $\frac{M_o}{I_o} = \frac{M}{I}$

Then $E\frac{d^2y}{dx^2} = \frac{M}{I} = \frac{M_o}{I_o}$

$$E\frac{dy}{dx} = \frac{M_o}{I_o}x$$

if origin is at fixed end.

$$Ey = \frac{M_o}{I_o}\frac{x^2}{2}$$

Maximum deflection of cantilever of uniform strength is at $x = l$, and equals $\frac{M_o l^2}{2I_o}$.

$$M_o = W_1 l,$$

$$\text{And } \therefore \delta_1 = \frac{W_1 l^3}{2I_o}$$

Where δ_1 is deflection of portion of spring of uniform strength.

If f_1 is skin stress in this cantilever,

$$\frac{M_o}{I_o} = \frac{f_1}{\frac{t}{2}}$$

$$\therefore f_1 = \frac{W_1 lt}{2I_o}$$

$$= \frac{6W_1 l}{n_1 bt^2}$$

First Case.—If the curvature of the portion of uniform cross-section is the same as the curvature of the portion of uniform strength initially, that is, before the load is applied, the band exerts no initial stress between the leaves. Then

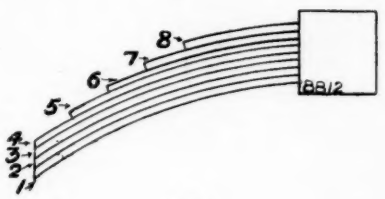

FIG. 2.

the deflections of the two portions will be the same. That is,

$$\delta_1 = \delta_2$$

or

$$\frac{W_2 l^3}{3EI_2} = \frac{W_1 l^3}{2EI_o}$$

$$\frac{W_2}{W_1} = \frac{3}{2}\frac{I_2}{I_o}$$

$$= \frac{3}{2}\frac{n_2 bt^3}{n_1 bt^3}$$

$$= \frac{3}{2}\frac{n_2}{n_1}$$

The skin stresses have the ratio

$$\frac{f_2}{f_1} = \frac{\frac{6W_2 l}{n_2 bt^2}}{\frac{6W_1 l}{n_1 bt^2}} = \frac{W_2}{W_1}\frac{n_1}{n_2} = \frac{3}{2}$$

That is, the skin stress in the portion of the spring of uniform cross-section is one and half times the skin stress in the portion of uniform strength. This is not desirable.

Second Case.—If the deflections are equal, but the number of plates in each portion of the spring is so arranged that the loads borne by each portion are equal.

That is $\delta_1 = \delta_2$,
and $W_1 = W_2$.

Then we have,

$$\frac{W_1 l^3}{3EI_2} = \frac{W_2 l^3}{2EI_o}$$

$$\frac{l^3}{3EI_2} = \frac{l^3}{2EI_o}$$

$$\frac{I_o}{I_2} = \frac{3}{2}$$

$$\frac{n_1 bt^3}{n_2 bt^3} = \frac{3}{2}$$

$$\frac{n_1}{n_2} = \frac{3}{2}$$

That is, the number of plates in the portion of uniform strength must be one and half times the number of leaves in the portion of uniform cross-section.

Third Case.—The best method of designing a semi-elliptic spring, however, is to so arrange the initial curvature of the portion of uniform strength that on the application of the load we get a constant skin stress throughout the spring.

$$\delta_2 = \frac{W_2 l^3}{3EI_2}$$

and $$\delta_1 = \frac{W_1 l^3}{2EI_o}$$

Also $$W_1 = \frac{n_1 b t^2 f_1}{6l}$$

and $$W_2 = \frac{n_2 b t^2 f_2}{6l}$$

Then $$\delta_1 = \frac{n_1 b t^2 l^3 \times 12 f_1}{6 \times l \times 2E n_1 b t^3}$$

$$= \frac{l^2 f_1}{Et}$$

and $$\delta_2 = \frac{n_2 b t^2 l^3 . 12 . f_2}{6l . 3 . E n_2 b t^3}$$

$$= \frac{2}{3}\frac{l^2}{Et} f2$$

and $f_1 = f_2$ if skin stress is constant.

Hence $$\frac{\delta_1}{\delta_2} = \frac{3}{2}$$

That is, if the skin stress is to be constant, the deflection of the portion of spring of uniform strength must be one and a half times the deflection of the portion of uniform cross-section.

Hence, if the curvature of the two portions is so arranged that there is a space between the leaves 3 and 4 equal to one half the final deflection of the combined spring and then the band is applied, the final deflection when the load comes on will bear the ratio of 3 to 2, and the skin stress will be uniform throughout the spring.

To find the number of leaves to carry a given load: $2W$ at the centre, having given the thickness of each leaf and assuming the curvatures to fulfil the above condition,

$$W = W_1 + W_2$$

and $$f_1 = \frac{6W_1 l}{n_1 b t^2}$$

$$f_2 = \frac{6W_2 l}{n_2 b t^2}$$

and $$f_1 = f_2$$

$$\therefore \frac{6W_1 l}{n_1 b t^2} = \frac{6W_2 l}{n_2 b t^2}$$

$$\therefore \frac{W_1}{W_2} = \frac{n_1}{n_2}$$

That is, load borne by each portion of the spring is directly proportional to the number of leaves in each portion.

$$W_2 = \frac{n_2}{n_1} W_1$$

and $$W_1 = (W - W_2)$$

$$\therefore W_2 = \frac{n_2}{n_1}(W - W_2)$$

$$W_2\left[1 + \frac{n_2}{n_1}\right] = W\frac{n_2}{n_1}$$

$$\therefore W_2 = \frac{\frac{n_2}{n_1} W}{\left(1 + \frac{n_2}{n_1}\right)}$$

Now $$W_2 = \frac{n_2 b t^2 f}{6l}$$

Hence $$t = \sqrt{\frac{6lW_2}{n_2 bf}}$$

$$= \sqrt{\frac{6Wl}{n_1\left(1 + \frac{n_2}{n_1}\right) bf}}$$

$$= \sqrt{\frac{6Wl}{(n_1 + n_2)\, bf}}$$

Or if P = central load = $2W$

then $$t = \sqrt{\frac{3Pl}{(n_1 + n_2)\, bf}}$$

Hence $$t^2 = \frac{3Pl}{(n_1 + n_2)\, bf}$$

$$\therefore (n_1 + n_2) = \frac{3Pl}{t^2 bf}$$

The number of leaves in each portion may be arranged as may be most convenient. The lengths of the leaves in the portion of uniform strength are found as follows:—

Draw the bending moment diagram or the cantilever of uniform strength.

Load borne by this cantilever is—

$$W_1 = \frac{\frac{n_1}{n_2}(W)}{\left(1 + \frac{n_1}{n_2}\right)}$$

Bending moment at fixed end of cantilever is $W_1 l$.

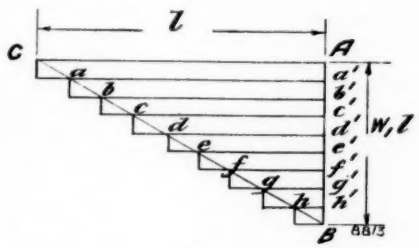

FIG. 3.

Draw $A\,C = l$ and $A\,B = W_1 l$.

Then $A\,B\,C$ is bending moment diagram for the portion of spring of uniform strength.

Divide $A\,B$ into n_1 parts, and erect rectangles as shown at $a_1\ b_1\ c_1$, &c.

Then the lengths of the members of the cantilever of uniform strength will be proportional to—

C	A
a	a^1
b	b^1
c	c^1
d	d^1
e	e^1 &c.

The first leaf $C\,A$ of the uniform strength portion will be, of course, equal to l, as will also the n_2 members of the portion of uniform cross-section.

If the above rules are followed, and the curvature of the portion of uniform strength is so arranged that there is a space (equal to $\frac{1}{2}$ the final deflection under load) before the band is applied, the spring will be strong enough to sustain a load of $P = 2W$ at the centre and will have a uniform skin stress throughout.

To find the final deflection of the combined spring:

Let Mc be bending moment at fixed end, and Ic = moment of inertia at fixed end.

$$Mc = Wl.$$

$$Ic = \frac{(n_1 + n_2)\,bt^3}{12}.$$

Taking origin at fixed end—

$$E\frac{d^2y}{dx^2} = \frac{Mc}{Ic}.$$

$$E\frac{dy}{dx} = \frac{Mc}{Ic}x + o.$$

$$Ey = \frac{Mc}{Ic}\frac{x^2}{2}.$$

Maximum deflection occurs when $x = l$.

Let δ = this deflection.

$$\text{Then } \delta = \frac{Mc}{Ic}\frac{l^2}{2E}$$

$$= \frac{Wl^3}{2IcE}$$

$$= \frac{6Wl^3}{(n_1 + n_2)\,bt^3E}.$$

Hence space between the two portions of the spring before band is applied should be—

$$\frac{3Wl^3}{(n_1 + n_2)\,bt^3E} \text{ inches}$$

$$= \frac{3Pl^3}{2(n_1 + n_2)\,bt^3E} \text{ inches}$$

if the skin stress is to be uniform throughout the spring.

Numerical Example.—Design a semi-elliptic spring to resist a load of 5 tons at the centre. The skin stress is to be uniform throughout the spring and is not to exceed 15,000 lbs. per sq. in. Total length of spring, 3 ft. Width of band at centre, 6 ins. Thickness of plate = ·6 in. Breadth of spring 6 ins.

$$P = 5 \times 2{,}240 \text{ lbs.}$$

$$f = 15{,}000 \text{ lbs. per sq. in.}$$

$$l = \frac{L}{2} - \frac{w}{2} = 18 \text{ in.} - 3 \text{ in.} = 15 \text{ in.}$$

$$(n_1 + n_2) = \frac{3 \times 5 \times 2{,}240 \times 15}{(\cdot 6)^2 \times 6 \times 15{,}000}$$

$$= 16 \text{ nearly.}$$

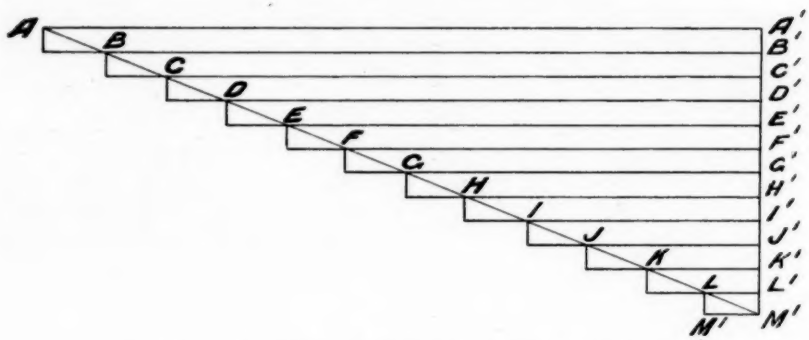

FIG. 4.—BENDING MOMENT DIAGRAM FOR CANTILEVER OF UNIFORM STRENGTH.

$A'M' = W_1 l = 28\frac{1}{8}$ inch tons.

Let there be twelve leaves in the portion of uniform strength, and four in the portion of uniform cross-section.

Space between the two portions of spring, before load is applied, must be

$$\frac{3 \times 5 \times 15^3}{2 \times 16 \times 6 \times (\cdot 6)^3 \times 13{,}000}$$

if E is taken as 13,000 tons per sq. in.

$$= \tfrac{1}{10} \text{ in. about.}$$

$$\frac{W_1}{W_2} = \frac{n_1}{n_2} = \frac{3}{1}$$

$$\therefore W_2 = \frac{1}{3} W_1$$

and $$(W_1 + W_2) = W$$

$$\therefore W_1 + \frac{W_1}{3} = W$$

$$\therefore W_1 = \frac{3}{4} W = \frac{3}{4} \times \frac{5}{2} = \frac{15}{8} \text{ tons.}$$

$\therefore$ Bending moment at end of cantilever of uniform strength is $\frac{15}{8} \times 15 = 28\frac{1}{8}$ inch tons.

Draw bending moment diagram as shown in Fig. 4.

From this diagram we obtain the lengths of the different leaves of the cantilever of uniform strength as follows:—

AA ft.	= 15 in.	GG ft.	= $7\frac{5}{8}$ in.
BB ft.	= 14 in.	HH ft.	= $6\frac{3}{8}$ in.
CC ft.	= $12\frac{7}{8}$ in.	II ft.	= $5\frac{1}{8}$ in.
DD ft.	= $11\frac{1}{2}$ in.	JJ ft.	= $3\frac{3}{4}$ in.
EE ft.	= $10\frac{1}{4}$ in.	KK ft.	= $2\frac{5}{8}$ in.
FF ft.	= 9 in.	LL ft.	= $1\frac{1}{2}$ in.

Now length of each leaf is twice the length obtained from bending moment diagram + the width of the band. Hence lengths of leaves of portion of uniform strength are:—

A	36 in.	G	$21\frac{1}{4}$ in.
B	34 in.	H	$18\frac{3}{4}$ in.
C	$31\frac{3}{4}$ in.	I	$16\frac{1}{4}$ in.
D	29 in.	J	$13\frac{1}{2}$ in.
E	$26\frac{1}{2}$ in.	K	$11\frac{1}{4}$ in.
F	24 in.	L	9 in.

Modern Locomotive Construction.

By J. G. BARWICK SAMS.

The first instalment of this article appeared in the July issue. In this number the author considers the subject of boiler construction and the erection of the frames. The concluding portion will deal with the erection of the running parts. (Ed. *E.R.*)

BOILER.

ENGLISH locomotive boilers are all of the one standard type, known as the locomotive type, having a more or less rectangular fire box, containing an inner fire box, and a horizontal barrel, containing the fire tubes. The size of the boiler is limited by the diameter of driving wheels and by the loading gauge.

The usual practice in boiler building is as follows:—The barrel is built from three plates bent into shape cold by passing through three rolls, the longitudinal seams being strapped and treble riveted, while the circumferential seams are lapped, by making each of the three rings to fit inside the other telescopic fashion, and double riveted. An angle iron riveted round the front end of the barrel fixes the barrel to the front plate or tube plate, Fig. 6 (*B*), which tube plate is pierced for the tubes, and is angled at the bottom edge to rest on the cylinder casting. The outer fire box consists of wrapper plate, throat plate and back plate.

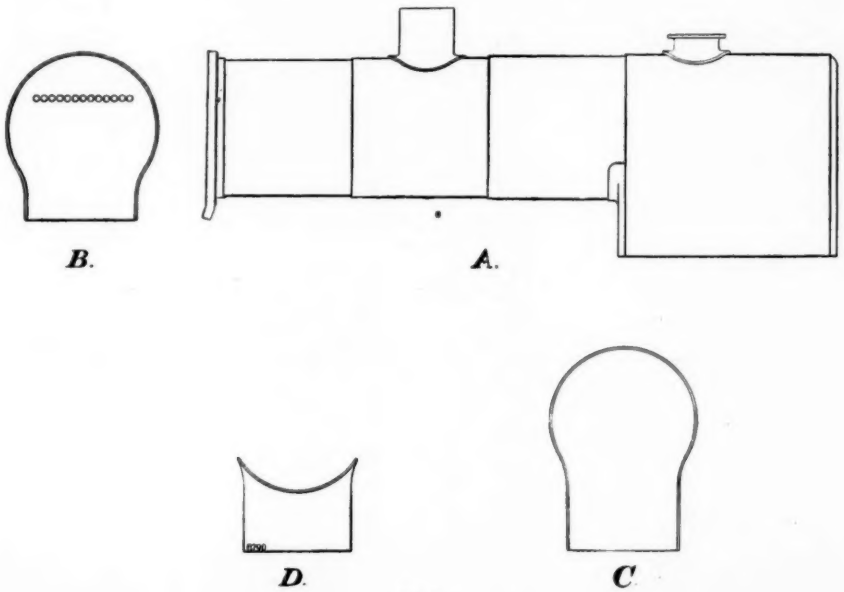

FIG. 6.

The wrapper plate, Fig. 6 (*C*) is riveted to the rearmost end of the barrel around the top half (where it is radiused to suit). The throat plate Fig. 6 (*D*) is riveted to the bottom half of the barrel end and along its sides, to the wrapper plate. The back plate is fitted in to the wrapper plate at the back and contains the fire door hole.

The inner fire box, which contains the furnace, is always of good quality copper about $\frac{3}{4}$ in. thick, and is in the form of a parallelopiped; it is made in the same way as the outer box, from three plates, the wrapper plate forming the sides and top, and a back and a front plate, the front plate being the rear tube plate.

A space of 2 to 3 in. is always left between the outer and inner boxes round the sides, back and front, and as all flat parts must be stayed, it is needful to stay the two boxes together by stays; usually 1 in. diam. of copper screwed all along with a thread of 12 to the inch. The corresponding stay holes in the two boxes are tapped at once by a long tap to ensure fairness of thread, and the stay run in and hammered over, inside and out. Outside ends can be machine riveted, but inside ends (exposed to the fire) are best hand-riveted, as the machine riveting is slightly irregular. The space between the two boxes at the bottom is filled by a forged ring of mild steel, rectangular in section, termed the foundation ring. This ring is held in position by rivets passing through the ring and through the two boxes.

The crown, or top of the inner fire box, being flat, has to be efficiently stayed either by direct stays as the side stays or by girder stays, to which the crown is secured at intervals by set bolts. These girder stays or roofing bars are cast in steel with lugs provided on their lower edge for the reception of the set bolts mentioned above. Each end of the girder is prolonged down to rest on the edge of the vertical sides of the inner box, and in addition, they are usually held up about the middle by steel slings depending from angle irons riveted round the inside of the outside wrapper plate (Fig. 7).

It will be seen that considerable flanging and shaping is necessary in building a boiler; and this is always done by hydraulic presses and standard dies. Now when steel is to be altered in shape suddenly, as in a press, it is very necessary that it should be properly heated to about a cherry-red, pressed out quickly, and carefully annealed. Directly the plates turn blue, all work on them must instantly be stopped, and the plates re-heated, otherwise they are *certain* to crack. But, in the case of a straight bend, *i.e.*, bending a plate to a circular shape for the barrel, or bending the middle of a wrapper plate into a circle of large radius, this can be done cold. All holes are drilled in the plates before flanging or bending, so that, directly the shaping is finished the

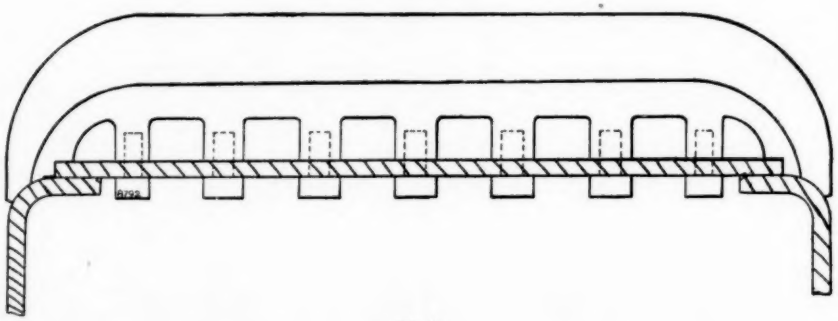

FIG. 7.

plates can be bolted up preparatory to riveting.

Boiler steel is usually Siemens-Martin open-hearth acid steel with about one per cent. of carbon; its average tensile strength is about 26 tons, and it has the advantage of being equally strong transversely as well as longitudinally, which iron has not.

In America steel is *always* used for inner fire-boxes, but British engineers invariably use copper as being the cheapest in the end. The tubes are usually of brass or copper, sometimes of steel, and are secured at each end by expanding them into the tube plates and bending the ends over. A slightly taper ferrule is driven into the fire box end of the tube to preserve them a little from the fierce heat of the furnace.

ERECTING.

The first thing in erecting is to assemble the frames and stays together. The two main frames are stayed together in front by the cylinders casting, at the rear by the foot plate casting, and in between by three ordinary plate stays, one of which forms the motion plate, the support for the back end of the guide bars.

The frames are brought into the shop and the hornblocks are bolted to them by temporary bolts (it is understood that all holes have been previously drilled one-eighth inch under standard). There is a flange on the hornblocks that fits up to the gaps in the frames, so that it cannot be bolted up wrongly. All centre lines are marked out now with a scriber and small centre dabs put to indicate the line. The vertical lines through the axle centres are brought up and produced over the top edge of the frames, and a centre dab put in the middle of the edge. The frames are now placed roughly in position in screw-stands, which are shown in Fig. 8, and levelled off carefully, both longitudinally and across, with a spirit level, the adjustments being made by means of the screws in the stands.

The cylinders casting, stay plates and motion plates have their centres marked

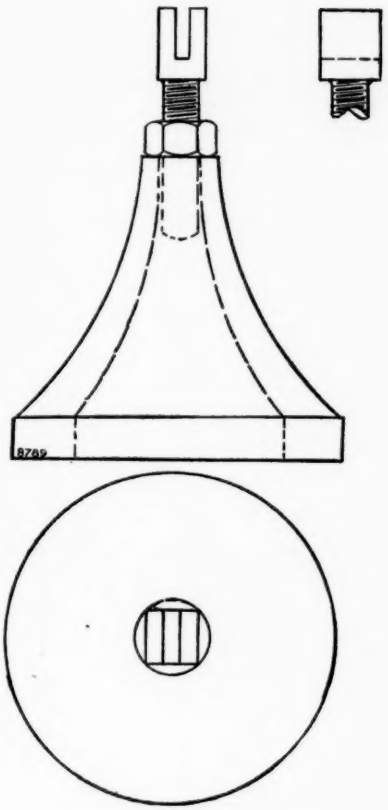

FIG. 8.

on them as follows: Strips of iron are inserted and jammed in the bore of the cylinders (Fig. 9), and the true centre marked with a dab, and a straight-edge of the correct length is clamped so that its upper edge is correctly coincident with the cylinder centres. Another straight-edge is fixed across the back end of the cylinders in a similar manner. A trammel is made (*B*, Fig. 9), with a true edge and a sharp point at each end in truth with the edge.

The motion plate, which at one time was a plate, is now generally a steel casting with lugs on it to which the

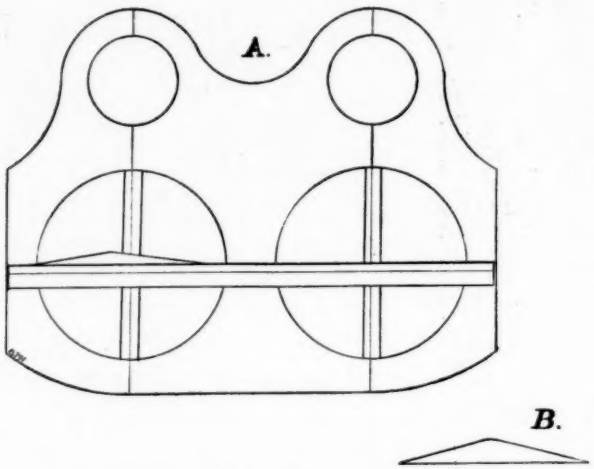

FIG. 9.

guide bars are attached; if strips of iron be inserted and jammed in between these lugs, the centres can be marked on the strips and the straight end clamped as in the case of the cylinders.

Having got the centres, the cylinders can be lifted into place and temporarily bolted up, also the motion plate, stays and foot plate casting.

Now the frames can be truly set in respect to each other. This is done by levelling them off with a spirit level to bring their edges true, and by trammelling with a long trammel bent at each end from, say, the centre dab in the middle of the top edge centre line of the right-hand leading axle to the similar centre dab on the left-hand driving axle (see dotted lines, Fig. 1), setting the trammel, and trammelling this time from the right-hand driving axle centre dab to the left-hand leading axle centre dab. If these two distances be made the same by altering the position of one of the frames if necessary, they will form the diagonals of a square, whose sides are the two centre lines of the leading and driving axles and the piece of each frame between them, and given equal diagonals, it is evident that each frame is at right angles with the axle centres, and therefore in its right position. If a straight-edge be held across the faces of each corresponding pair of hornblocks they should show up in line, given accuracy in machining.

Having now got the frames set we can proceed with trueing the cylinders.

Referring to Fig. 9, which shows the straight-edge in place: If the edges of these straight-edges (back and front) be made to correspond exactly with the scribed centre lines on the frames, the cylinders will be in their true position. To secure this, the little trammel (*B*, Fig. 9) is used; its true edge being placed on the centre line edge of the straight-edge, and the position of the cylinders altered until its point coincides the engine centre lines on the frames, when of course the cylinders will be set true. They may then be bolted up very tight by the temporary bolts (which have had to be slackened to allow the cylinders to be set), and the bolt holes through frames and cylinder-castings rose-bitted out one at a time. As soon as a hole is done, its bolt, which is made a driving fit, must be driven home with a 7 lb. hammer and bolted up permanently. As fast as the permanent bolts are driven up the temporary ones are removed and their holes rimered, care being taken to always leave a sufficiency of bolts to hold the cylinders firmly.

The motion plate is set exactly in the same way and the holes rose-bitted for the permanent bolts. Inch to $1\frac{1}{8}$ in. bolts are generally used for bolting the cylinders to the frames, and they should be a good driving fit and the nuts screwed up hard.

The hornblock holes may now be rose-bitted out and all bolts driven in

and tightened up; some firms prefer the hornblocks hot-riveted, which is done by a hydraulic riveter.

The other plate stays are set up in position and held up by temporary bolts while they are set vertically by a plumb-bob, generally their top and bottom edges coincide with the top and bottom edges of the frames so that they are easily set, after which the holes are rose-bitted and cold rivets driven in and hammered over. By cold rivets it must be understood that turned driving fit rivets are meant. These rivets are usually employed where they are not likely to be ever removed for the purpose of executing repairs, &c. They must be well held up in order to make a sound job. The foot-plate casting may now be set, and the holes rose-bitted and the permanent bolts put in. This casting always finishes flush with the ends of the frames, and its surface is flush with the top edges of the frames, so that it is easily set.

The foot-plating round the engine can now be set up in place and hot-riveted up. This plating consists of an outside angle iron parallel with the frames and held up by right-angled brackets at intervals, over which the actual plating is laid, suitable spaces being cut out for the wheels, and splashers of cast iron being fitted and bolted up. In the case of large diameter wheels, say 5 ft. and upward, the splashers are usually built up of light plates and angle irons bent to the required radius. The sand boxes are also attached to the foot-plating usually by $\frac{1}{2}$ in. bolts and nuts, also the footsteps for mounting to the cab. The usual height of the foot-plating round the engine is 4 ft. from rail level.

The positions of the brake hanger studs are now marked off, and the bolt holes (usually three in number), are drilled. These are heavy studs with flat bases, bolted up against the frame from which hang the brake block hangers, carrying the blocks for breaking the wheels.

All drilling, rose-bitting, &c., is done by compressed air or electric drills, hot riveting by hydraulic power, but countersunk riveting, hot or cold, has to be done by hand. Cold-riveting also must be done by hand. Of course it would not pay to bring up a machine to rivet isolated rivets, these being hand-riveted.

The frame is now finished, and ready for the boiler to be tried on.

The boiler is fixed at the front end by bolting the tube plate to the top of the back end of the cylinder casting, but it must be left free at the rear end, in order that it may expand and contract without racking the frames. To ensure freedom and at the same time to hold the boiler down, stout angle irons are riveted on to each side of the outer fire box, and these angle irons rest on large flat castings fixed to the frames on each side. Caps are bolted down on to these castings, but are cut away clear so as not to seize on the angle irons and thus grip the boiler. It is a good plan to put brass strips between angle irons and castings, as these provide a sort of bearing for the boiler angles.

The edge of the tube plate may have to be chipped to suit the bedding on the cylinder casting, and when correct the bolts may be put in and screwed tight.

(To be concluded.)

NORTHAMPTON POLYTECHNIC INSTITUTE.—The recent provision of increased accommodation not having been found sufficient for the growing requirements of the "Northampton," an additional building is now being erected in the courtyard, and it is hoped that the greater part of it will be available immediately after Christmas. The new building will contain a lecture room larger than any of the existing lecture rooms, and also increased class room accommodation. It also provides for the enlargement of the mechanical engineering workshops and the mechanical laboratory, and includes new power laboratories. With this increased accommodation the work in the mechanical engineering department will be materially assisted.

In the new session the work of the day courses in engineering, technical optics and artistic crafts will be continued with minor developments on the lines which are now so well known.

In the evening classes the electrical engineering department announces a full sessional course in wireless telegraphy and telephony, the pioneer course held last May in this subject having proved to be very successful.

XUM

The Stress in Wire Ropes due to Bending.

By R. W. CHAPMAN.

Unwin states that a wire rope used as a belt is subjected to three different straining actions. Firstly, there is longtitudinal tension, due to the tightness with which the belt is strained over the pulleys, to the weight of the rope and to the power transmitted. Secondly, there are stresses of tension and compression in the part of the belt due to bending at the curve of the pulley. Lastly, there is a stress due to the centrifugal action of that part of the belt which is being bent. Mr. Chapman, in the following paper, examines in detail the stresses due to bending. (Ed. *E R*)

WITH the progress of modern mining engineering, one of the features of which is the mining from greater depths and the hoisting of greatly increased loads, it becomes a matter of increasing importance that the engineer should be able to determine with some degree of precision the character of the strains on the hauling rope. But a wire rope is a complex structure; it is made up of spirally-twisted strands, which in turn are composed of spirally-twisted wires, and it is not particularly easy to say exactly what happens when this doubly-twisted structure is wound round a drum or over a pulley. The difficulty has been generally evaded in ordinary practice by the simple device of adopting a large factor of safety for the rope—usually from eight to ten—a factor which experience has shown is sufficiently large to cover the bending strains set up when the rope is passed over pulleys of the sizes commonly considered appropriate. When, however, we add the stress due to the bending of the rope to the stress produced by the direct pull, we find that the actual factor of safety is usually more nearly four than ten, and frequently the bending stresses in the rope are so large compared with the simple tensile stresses that their neglect may easily lead to disastrous results when dealing with problems a little out of the common run.

Common Empirical Rules.—It is always recognised that the bending of a wire rope round too small a pulley is likely to do the rope serious injury, but as it is desirable in ordinary winding operations to reduce the inertia of the moving parts to a minimum, the diameters of sheave and drum are commonly made as small as is considered right for the size of rope to be used. Various empirical rules have been given by different makers and writers for determining the minimum size of drum over which a rope may be wound. Bullivant's catalogue, for instance, states that sheaves and barrels should be about thirty times the circumference of the ropes used. Lupton ("Mining") says that the diameter of the drum in feet should $= 0{\cdot}7\ C^2$, where C is the circumference of the rope in inches. H. W. Hughes ("Text-Book of Coal-Mining") states that the diameter of the pulley should not be less than 100 times that of the rope. W. H. Tinney ("Gold Mining Machinery") gives the rule 1 ft. of pulley for each $\frac{1}{8}$-in. of rope, thus an 8-ft. pulley for a 1-in. rope. Such rules are obviously imperfect in that they take no account of the size of the component wires, and from ordinary experience we know that the smaller the diameter of the wires of which a rope is made the more flexible it is, and the smaller may be the diameter of the pulley over which it may be wound without injury. Thus a rope with nineteen wires to the strand is more flexible than one with seven wires to the strand. For general application the rule should obviously take into account the gauge of the wire composing the rope. Such rules are sometimes stated. Thus

Bucknall Smith* states:—"The gauge of the wire used in any mining rope should not exceed $\frac{1}{800}$th part of the diameter of the smallest pulley over which it has to run." J. B. Richards† says:—"Practice and good records made in wire rope transmission, as well as mathematical demonstration, show that the proper diameter of a sheave should be 115 times the diameter of a rope of nineteen wires to the strand, and 185 times the diameter of a rope of seven wires to the strand."

The Australian representatives‡ of Messrs. T. and W. Smith deduce from the practical rules of that firm that a 6-strand 7-wire rope requires a pulley whose diameter is equal to ten multiplied by rope circumference squared, and a 6-strand 19-wire rope requires a pulley whose diameter is equal to four multiplied by rope circumference squared.

Such rules as these, whilst useful in enabling us to decide approximately upon what experience has shown to be the proper size of pulleys to use with a given class of rope, do not help us in the determination of the actual stress upon the rope.

Formulæ for the Determination of Bending Stress.—Reuleaux, Rankine, Unwin, and other writers on applied mechanics give an investigation into the character of the bending stresses in wire ropes, which results in the following formula:—

$$f = \frac{Ed}{D}$$

Where d = diameter of a component wire.
D = diameter of drum.
E = modulus of elasticity of material of wire.
f = stress per sq. in. due to bending.

This result, however—which is the ordinary text-book formula—is based upon reasoning that would be precisely the same if the rope were made up of a bundle of parallel wires. As the very object of the spiral construction is to give flexibility, we have reason to suppose that the bending stresses in practice will be less than those given by this equation. This idea is strengthened when we seek to apply the calculation to common mining practice. For instance, to take an example at random from Broken Hill, in one mine the rope in use is one of 3 in. circumference, consisting of six strands, Lang lay, each strand consisting of an iron wire core ·115 in. diameter, and six steel wires, each ·107 in. diameter. The total load—cage, trucks, ore, and rope—amounts to 5,428 lbs. The guaranteed breaking stress of the rope is 26½ tons, which gives an apparent factor of safety, without taking bending stresses into account, of nearly eleven. The rope, however, is wound round a 9-ft. drum, and, according to the preceding formula, the bending stresses set up by this, taking E as 28,500,000 lbs. per sq. in., amount to over 28,000 lbs. per sq. in. The stresses due to the direct load come to only 14,000 lbs. per sq. in., so that the total stress on the wires is over 42,000 lbs. per sq. in., and the actual factor of safety is only 3·6 instead of eleven. Now, when we consider that this load is continually being taken off and on, and that the rope must at times sustain stresses considerably higher than these, owing to the effects of jerks, it seems extraordinary that with a factor of safety of only 3·6, which means that the wires must be at times strained perilously near to the elastic limit, the rope should last as well as experience proves it to do. And this is no exceptional case. It is easy to find examples from ordinary practice where the factor of safety, taking into account the stresses due to bending as computed according to this formula, is even less than 3·6, and where in consequence the wires are strained almost right up to the limiting strength of the material for repeated loads. And yet, under such conditions, the rope wears well.

Mr. W. Hewitt, in his book on "The Applications of Wire Ropes to Transportation," published by the Trenton

* *Mining Journal*, 6th June, 1896.
† *Mines and Minerals*, April, 1904.
‡ *Australian Mining Standard*, July, 1907.

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Iron Co., U.S.A., gives tables of the bending stresses on ropes with nineteen wires and ropes with seven wires to the strand, the tables being computed from the formula—

$$k = \frac{Ea}{2{\cdot}06\,\frac{R}{d} + C}$$

In which

k represents the bending stress in lbs.
E, the modulus of elasticity, taken as 28,500,000.
a, the aggregate area of the wires in sq. ins.
R, the radius of the bend in ins.
d, the diameter of the individual wires in ins.
C, a constant depending on the number of wires in the strand.

The values of d and C used in the computations are—

	7-wire Rope.	19-wire Rope.
d =	$\frac{1}{9}$ diam. of rope ...	$\frac{1}{15}$ diam. of rope.
C =	9·27 ...	15·45

No particulars are given as to the way in which this formula is obtained, but if expressed in the same way as Reuleaux's formula it becomes

$$f = \frac{{\cdot}97\ Ed}{D + 9d} \text{ or } \frac{{\cdot}97\ Ed}{D + 15d}$$

according as the strands are of seven or nineteen wires. It is thus apparently based upon the same formula, but in making the computation the diameter of the drum is increased by that of the rope, and the stresses so obtained are reduced by multiplying by ·97. This formula thus gives slightly lower results for the bending stresses than that of Reuleaux, but not to any appreciable degree.

In 1902 Josef Hrabak published his book entitled "Die Drahtseile," in which he enters into an elaborate investigation of the magnitude of the stresses set up by the bending of wire ropes. As the result he is apparently able to reconcile the discrepancy between theory and practice by determining that the bending stresses are less than half those computed by the old formula. Taking a wire rope in which the modulus of elasticity of the material of the wires is 28,500,000 lbs. per sq. in. and the angle of lay is 18°, he finds

$$f = \frac{{\cdot}44\ Ed}{D}$$

—*i.e.*, the stress is ·44 times that computed by the Reuleaux formula.

Hrabak's reasoning is as follows. He denotes by—

E_0, the modulus of elasticity of the straight wires.
E, the modulus of elasticity of the rope.
E^1, the modulus of elasticity of the wires *as laid in the rope*.

Experiments on ropes subjected to direct tension show that a rope stretches nearly three times as much as it would do if made up of a bundle of parallel wires, and as an experimental average we may take—

$$E = 0{\cdot}36\ E_0$$

Hrabak then enters into a mathematical investigation, in which, *on the assumption that the stretching of a rope under a straight pull is wholly due to the elongation of the wires*, and neglecting the decrease in the diameter of the rope, he obtains the relation between E^1 and E.

$$E^1 = E \sec.^2 a. \sec.^2 b,$$

where a is the angle of lay of the wires in the strands,
and b is the angle of lay of the strands in the rope.

Thus he obtains $E^1 = 1{\cdot}222\ E = {\cdot}44\ E_0$, and he then uses this value for the modulus of elasticity in the Reuleaux formula, with the result already stated.

In the very able discussion that took place on Behr's paper on "Winding Plants for Great Depths,"* Hrabak's results were quoted with the greatest respect, but the writer has no hesitation in saying that the reasoning is entirely inadmissible. From first principles there appears to be no reason whatever why the modulus of elasticity of the wires as laid in a rope should be different

* "Proceedings South African Association of Engineers," vol. vi.

from that of the same wires when straight. The material of the wires is not altered by winding them into a rope, and certainly nothing is done to them that would lead us to suppose that the wires in a rope would stretch twice as far as they would otherwise do under the same stretching force, as Hrabak's result would indicate. The mathematical reasoning, by which the relation between E^1 and E is obtained assumes, as the writer has already stated, that when a rope is stretched by a direct pull the elongation is wholly due to the stretching of the wires. But the lengthening of a spiral spring under tension is not due to the elongation of the wire at all, or only to an indefinitely small extent. It is due principally to the separation of the coils, combined with a reduction in diameter. And precisely the same action takes place in a rope under tension, to a less degree than with a single spiral spring, because of the tight packing together of the various coils; but a rope elongates more than a bundle of parallel wires entirely because of this action, and not because the wires in a rope stretch more than straight wires would do under the same tension. Hrabak's mathematics, in which he examines the relation between the elongation of a rope consisting of a single strand and the elongation of the component wires, might be applied just as well to the elongation of a single spiral spring. Applied in that way they are obviously wrong, and the same error vitiates his entire argument. Throughout his work he has assumed that the stretching of a rope under tension is entirely due to the elongation of the wires, and in his estimate of this he has altogether neglected any reduction in the diameter of the rope.

Change in Curvature of the Wires of a Rope when Bent Round a Pulley.—In order to determine the stresses in the wires of a bent rope, the first thing that seems to be necessary is to find out exactly how much the wires are bent when the rope is wound round a drum of given radius. The Reuleaux formula assumed that the change in curvature of the wires was the same as the change in curvature of the rope. We will now examine how far this assumption is correct. This can be most simply done by an application of a well-known theorem in solid geometry according to which the radius of curvature of any normal section of a surface is given by the formula

$$\frac{1}{r} = \frac{\sin.^2 a}{R_1} + \frac{\cos.^2 a}{R_2}$$

where R_1 and R_2 are the radii of curvature of the two principal normal sections and a is the angle which the normal section whose radius is r makes with the first principal normal section.

(*a*) Rope in the form of a single strand.

For greater simplicity we will consider first of all the case of a rope consisting of a single strand—that is, a rope made up of layers of wires wound spirally about a central core. In this case each wire forms a simple helix when the rope is straight. Let the diameter of one of the layers, measured to the middle points of the wires, be c, and let the helix formed by the wire make an angle a at any point with the axis of the rope. Then, when the rope is straight, this layer forms a cylindrical surface, and the intersection of this by a normal plane parallel to the axis gives a straight line, while the intersection by a normal plane perpendicular to the axis gives a circle of diameter c. Thus, applying the preceding formula, when the rope is straight the radius of curvature of the wire at any point is given by

$$\frac{1}{r} = \frac{2 \sin.^2 a}{c}$$

This is the initial radius of curvature given to the wire in the manufacture of the rope.

Now suppose the rope is bent round a pulley of diameter D.

If E C in Fig. 1 represents the axis of the pulley, the rope now forms the surface mathematically known as an "anchor ring" with E C as axis. The principal radii of curvature of this surface at any point P are P O and P C.

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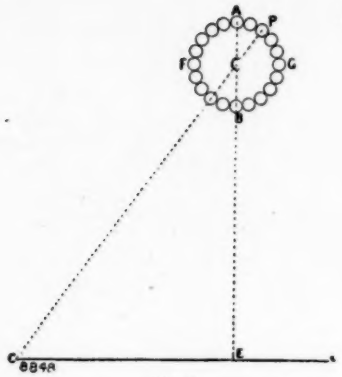

FIG. 1.

Therefore the curvature of the wire at P becomes

$$\frac{1}{r'} = \frac{2 \sin.^2 a}{c} + \frac{\cos.^2 a}{P C} = \frac{2 \sin.^2 a}{c} + \frac{2 \cos.^2 a}{(D + c) \sec. A + c}$$

where A denotes the angle A O P or C O E.

Thus the alteration in curvature of the wire at any point P, measured in the osculating plane of the wire, is given by

$$\frac{1}{r'} - \frac{1}{r} = \frac{2 \cos.^2 a}{(D + c) \sec. A + c}$$

or, neglecting c in comparison with D, as we may usually do in practice,

$$\frac{1}{r'} - \frac{1}{r} = \frac{2 \cos.^2 a}{D \sec. A}$$

For the radius of curvature at any point such as A, on the convex side of the rope, we have

$$\frac{1}{r'} = \frac{2 \sin.^2 a}{c} + \frac{2 \cos.^2 a}{D + 2c}$$

at any point such as B on the concave side of the rope

$$\frac{1}{r'} = \frac{2 \sin.^2 a}{c} - \frac{2 \cos.^2 a}{D}$$

the two principal radii of curvature of the surface being here in opposite directions.

At points such as G and F, midway between A and B, there is no alteration of the curvature of the wires, in the plane tangential to the wire and normal to the rope, due to the bending of the rope.

We thus see that the curvature of the wires on the side of the rope in contact with the pulley is decreased, for the wires on the outer surface it is increased, and midway between there is no change of curvature. The maximum change of curvature takes place at the points A and B, and is given by the formula

$$\frac{1}{r'} - \frac{1}{r} = \pm \frac{2 \cos.^2 a}{D}$$ neglecting c in comparison with D.

Of these two points, A and B, the greater alteration in curvature takes place at B, since its value at A is more exactly given by

$$\frac{1}{r'} - \frac{1}{r} = \frac{2 \cos.^2 a}{D + 2c}$$

In this work we have assumed that the wires of the rope still make an angle a with the axis of the rope after bending. This is not strictly correct; but, as we shall see presently, although bending of the wires also takes place in the plane tangential to the rope, there is no alteration of a due to bending at the points A and B.

(*b*) Ordinary form of rope, consisting of a number of strands, each strand wound as a helix.

Considering any one of the wires, let the diameter of the cylinder which the wire forms when originally wound into the strand be denoted by c, and that of the cylinder formed by the winding of the strand into a helix be denoted by e, measured to the centre of the strand.

Let a be the angular pitch of the wires in the strand, and b that of the strands in the rope.

Applying the same theorem as before, we have, when the rope is straight, for the radius of curvature of the wire at any point

$$\frac{1}{r} = \frac{2 \sin.^2 a}{c} + \frac{\cos.^2 a}{R_1}$$

where R_1 is the principal radius of curvature of the helix formed by the strand.

When the rope is bent round a pulley of diameter D

$$\frac{1}{r'} = \frac{2 \sin.^2 a}{c} + \frac{\cos.^2 a}{R_2}$$

R_2 being the new principal radius of curvature of the helix formed by the strand.

$$\therefore \frac{1}{r'} - \frac{1}{r} = \cos.^2 a \left(\frac{1}{R_2} - \frac{1}{R_1} \right)$$

Now, at the surface of the rope in contact with the pulley, and at the outside surface of the rope, we have

$$\frac{1}{R_1} = \frac{2 \sin.^2 b}{e}$$

$$\text{and } \frac{1}{R_2} = \frac{2 \sin.^2 b}{e} \pm \frac{2 \cos.^2 b}{D}$$

just as in the previous investigation.

Therefore, we get for the extreme changes in curvature normal to the rope surface—

$$\frac{1}{r'} - \frac{1}{r} = \pm \frac{2 \cos.^2 a, \cos.^2 b}{D}$$

It will be noticed that these expres sions for the changes in curvature of the wires hold good either for ropes wound on the ordinary lay or Lang lay.

Stress in Wires Due to Bending.—If the radius of curvature of a wire is changed from r to r', it is easily shown that the maximum stress set up in the wire due to the bending is

$$\frac{Ed}{2} \left(\frac{1}{r'} - \frac{1}{r} \right)$$

where d is the diameter of the wire.

Thus in this case the stress set up by bending becomes

$$\frac{Ed}{D} \cos.^2 a, \cos.^2 b.$$

If $a = b = 18°$, $\cos.^2 a$, $\cos.^2 b = \cdot 81$, and the bending stress then becomes $\cdot 81$ of that computed by the Reuleaux formula, and much greater than the estimate of Hrabak.

The writer can see no escape from this argument. It comes to this, that it is a deduction from pure geometry that the curvature of the outer wires of a rope is changed by the amount $\frac{2 \cos.^2 a \cos.^2 b}{D}$, when the rope is bent round a drum of diameter D, and this being so, the stress set up by this bending must be at least as great as we have computed.

Character of the Stresses in a Bent Helix.—We might expect to obtain further information as to the nature of the stresses in the bent rope by investigating the character of the stresses in the various parts of a helical wire, such as is formed by the core of a strand of a rope or by a helical spring, when the axis of the helix is bent into a circle. This will be the case if we take such a helix and subject the ends to equal and opposite bending moments. At any point on the bent helical wire the resultant action may be resolved into (1) a bending moment in the plane tangential to the cylinder enclosed by the helix, (2) a bending moment in the plane containing the wire at right angles to this tangential plane, (3) a twisting moment. We will call the convex side of the bent helix the top, and the concave side the bottom. Then it may be shown that the bending moment (1) is a maximum at the sides of the bent helix, and is nothing at the top and bottom. The bending moment (2), on the other hand, is nothing at the sides, and is a maximum at the top and bottom; while the twisting moment (3) is also nothing at the sides, and has its maximum value at the top and bottom. In all cases it will be found that the direction of the twist on the convex side of the helix is in the same direction as that of winding. Applying this result to the strands of an ordinary rope, we see that when a rope of ordinary lay is bent round a pulley, the strands are subject to a twist, which tends to loosen or untwist the wires of the strand on the convex side, and to twist the wires higher on the side in contact with the pulley. With a rope wound on the Lang lay, the reverse action takes place.

The stress in the wire of the bent helix is thus produced under the combined action of twisting and bending, and, applying the ordinary formula applicable in such cases, it may be shown that the maximum stress occurs,

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not at the top or bottom of the bent helix, but at the sides, where it is due wholly to bending in the tangential plane.

It is difficult to say, however, to what extent we can apply these results to the strands of an ordinary rope. In the straight rope, of course, each strand forms such a helix as we have been considering, but when the rope is bent the helix is not free to behave as it would do if the other strands were not closely packed in contact with it. If each wire of a rope were free to behave like a complex helical spring when the rope is subjected to a straight pull, it is easy to show that the amount of elongation of the rope under direct tension would be much greater than it actually is. Something of this action takes place, and accounts for the fact that a rope elongates much more than a bundle of parallel wires, but the action is not complete. And similarly it does not seem that we can draw any very definite conclusions as to the stresses in the wires of a bent rope by investigating the nature of the stresses in a single bent helix.

Conclusions.—The one definite result we seem to be able to deduce by mathematical investigation is what we have already obtained, that the curvature of the outer wires is altered by a definite calculable amount on bending the rope round a pulley, and the stress so set up we have seen to be

$$\frac{Ed}{D} \cos.^2 a, \cos.^2 b.$$

The result would be at fault if the shape of the section of the rope were changed from the circular to oval by bending, but on applying calipers to bent ropes the writer finds that this does not occur to any measurable degree unless the rope is bent to a far sharper curve than would be ever allowed in practice.

Taking the diameter of the wire as one-ninth the diameter of the rope for a rope of six strands of seven equal wires, we get, if $a = b = 18$ deg., the stress per square inch produced by bending such a rope of diameter x inches about a pulley of diameter D inches $= \frac{x}{D} \times$ 2,587,000 lbs., assuming that E = 28,500,000.

Similarly for a rope with six strands of nineteen equal wires, taking the diameter of each wire as one-fifteenth the diameter of the rope, we get the stress produced by bending—

$$= \frac{x}{D} \times 1{,}552{,}000 \text{ lbs.}$$

If the core of each strand is a wire of diameter d^1, since this forms a simple spiral, the stress in it will be $\frac{E^1 d^1}{D}$ $\cos.^2 a$. Thus if the internal and external wires are to be equally strained by bending of the rope, we should have the relation $E^1 d^1 = Ed \cos.^2 b$, or if $E^1 =$ E, d^1 should $= \frac{9}{10} d$.

With the ordinary construction of rope, where the core and outer wires of each strand are all of the same diameter, the core wire is subjected to bending stresses greater by about 10 per cent. than those in the outer wires if it is made of the same material. To obtain uniform bending stresses in all the wires with this construction, the material of the core wire should have a modulus of elasticity only about nine-tenths that of the outer wires.

Experiments on Bending of Ropes.—In making any experiments on the bending of ropes, we are at once confronted with the difficulty of making allowance for the internal friction between the wires of the rope, because it is possible to produce very different deflections with the same deflecting force. We may, however, practically eliminate this factor in experiment by the following method:—A short length of rope, A B, is securely clamped horizontally from A to C, and its end is subjected to deflecting forces, P, gradually applied first on one side and then on the other (Fig. 2). Plotting the relations between the deflecting force and the deflection produced as in Fig. 3, as we increase the deflecting force on the right from zero the diagram gives a curved line running from O to A.

Gradually reducing the force applied, we can lessen it quite considerably before there is any appreciable diminution in the deflection, owing to the effects of friction. The results are shown by the line A B and part of B C. When the deflecting force on the right has been reduced to zero, the force on the left is gradually increased to the same amount as the one on the right. The results when plotted complete the line B C. This force is then diminished to zero, and the force on the right again gradually increased to its former value. The relations between the force and the deflections produced are shown by the line C D A. The results thus plotted form an approximate parallelogram, B C and A D being nearly parallel straight lines for the greater portion of their length, and a repetition of the experiment traces out the same area again.

Making the forces go through a complete cycle in this way, the area A B C D of the resultant diagram represents the work that has been lost in internal friction of the rope. The beneficial effects of rope lubrication are shown by repeating the experiment with the same rope well lubricated. The diagram obtained under the altered conditions is shown by the corresponding dotted lines. The direction of B C and A D remains the same, but the area of the diagram, or the amount of work used up in internal friction, is greatly diminished. The lines A D and B C represent the relations between the deflecting force and the deflection produced when friction is acting first in one way and then in the other. The parallel dotted lines passing through O will then represent the relation between the force and the deflection when friction is eliminated.

To get a comparison of experiment with theory we may estimate the bending moment required to bend a rope to a radius R as follows:

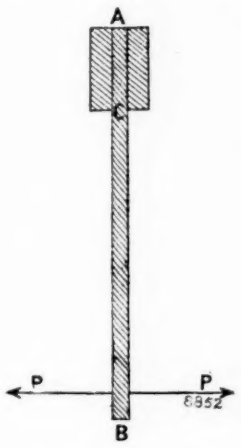

FIG. 2.

—If M be the bending moment applied to each wire, the resolved part of this parallel to the strand at the top of the convex side of the rope is M cos.*a*. Resolving this again parallel to the wire at the top of the convex side of the strand, we get M cos.*a*, cos.*b*. But we have seen that the change of

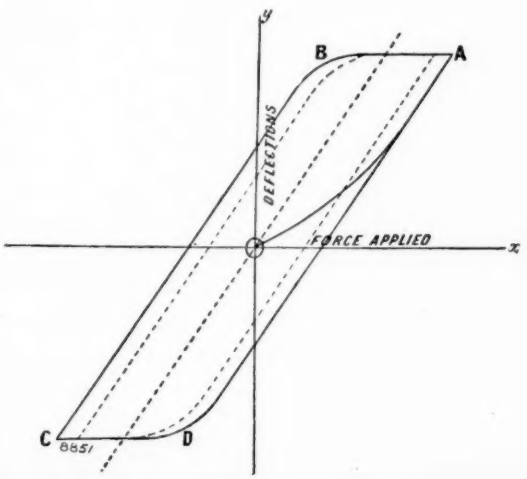

FIG. 3.

curvature of the wire at this point is $\frac{\cos.^2 a,\ \cos.^2 b.}{R}$

Therefore,

$$M \cos.a,\ \cos.b = \frac{E\ I \cos.^2 a,\ \cos.^2 b}{R}$$

$$\text{and } M = \frac{E\ I \cos.a,\ \cos.b}{R}$$

where I denotes the moment of inertia of the cross-section of each wire.

Similarly, if M^1 denotes the bending moment applied to each wire forming the core of a strand, we get

$$M = \frac{E^1\ I^1 \cos.a}{R}$$

Thus, if the rope consists of n strands of m equal wires each, with an additional wire core to each strand, the total bending moment that must be applied to bend this rope to a radius R is—

$$n\ m\ \frac{E\ I \cos.a,\ \cos.b}{R} + n\ \frac{E^1\ I^1 \cos.a}{R}$$

With this as a basis we may compute the deflection of the ropes under given deflecting forces in the same way as we compute the deflection of ordinary beams. The writer has made a considerable number of experiments with different classes of ropes in the manner described, and also by subjecting ropes to a uniform bending moment over a length of about 2 ft., and has found a close agreement between the observed and calculated deflections.

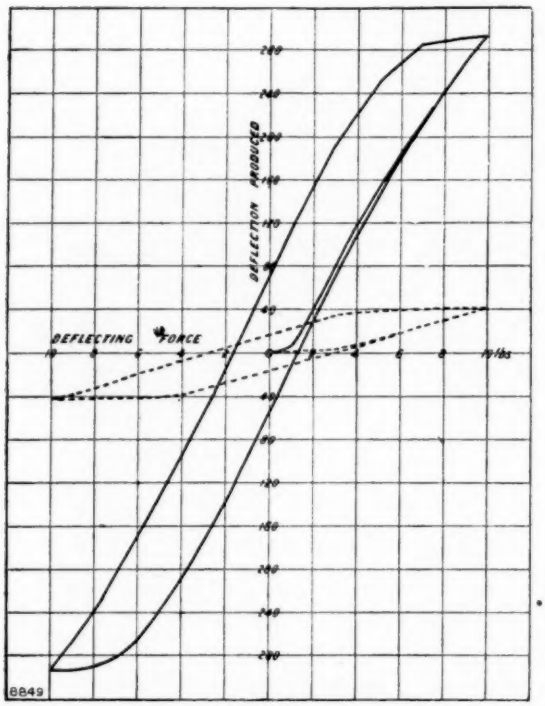

FIG. 4.

Showing relation between deflecting force and deflection produced in the case of flexible wire rope, 4 in. circumference, consisting of 6 strands of 12 wires, with hemp core. The vertical scale represents deflections in 32nds of an inch. The horizontal scale gives the deflecting force in lbs.

In illustration is appended experimental diagrams for two of the ropes tried. Fig. 4 shows the deflection diagram for a flexible steel wire rope 4 in. in circumference, consisting of six strands of twelve wires, with a hemp core to the rope and hemp cores to the strands. The rope was of ordinary lay, diameter of wires ·082 in., and the angle of lay $18\frac{1}{2}°$. In the large diagram the length of the free portion of the rope was 26 in., and the force applied 2 in. from the free end, or 24 in. from the fixed support. In the dotted diagram the length of the free portion was 14 in., and the force applied 12 in. from the support. With the large deflections obtained in the first case there is a considerable variation from the straight line in the side of the diagram, so that only the portion near to O is used. But in the second case, with smaller deflections, the parallelogram formed is well marked. In these experiments the

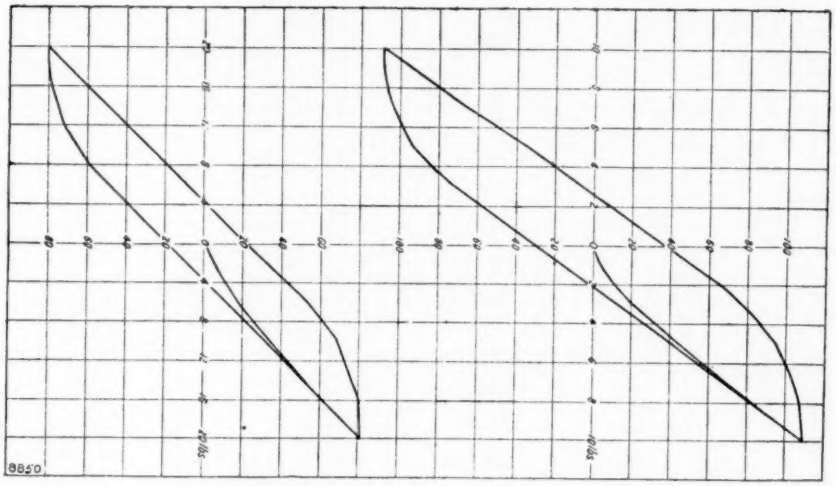

FIG 5.

Showing deflection diagrams for a Lang lay rope, $3\frac{3}{8}$ in. circumference, consisting of 6 strands of 7 equal wires. Diameter of each wire 116 in. The vertical scale gives deflections in 32nds of an inch.

observed ratios between the deflection in inches and the deflecting force in pounds were 1·31 and ·19 respectively, and the calculated ratios were 1·28 and ·185.

Fig. 5 shows similar diagrams for a Bullivant's Lang lay rope, $3\frac{3}{8}$ in. circumference, consisting of six strands of seven equal wires. Diameter of each wire ·111 in. In the upper diagram the distances of the point of application of the force and of the free end of the rope from the fixed support are 21 and 23 in. respectively. In the lower diagram these distances are 14 and 16 in. The observed ratios between deflection and deflecting force in these cases were ·45 and ·16, and the computed ratios were ·44 and ·14 respectively. These are typical of other results obtained.

The amount of work lost in friction varied considerably, but on the average the amount of work that had to be done to bend the rope and then get it straight again, as computed from the resultant diagrams, was about double that required to bend the corresponding frictionless rope.

The experimental method described furnishes us with a rational method of comparing the "flexibilities" of ropes. The more steeply inclined the central dotted line in Fig. 3 is to Ox for a given length of rope, the more flexible is the rope.

Work Done in Bending a Rope Round a Pulley.—It is of some interest to make an estimate of the work done in bending a wire rope round a pulley, in order to determine what amount of work is lost in this way in hauling operations. The work done in bending a rope of length l to a curve of radius R is $\frac{1}{2}$ M. $\frac{l}{R}$ Thus, if a rope is drawn over a pulley of radius R at the rate of l ft. per minute, since owing to internal friction work is done first in bending and then in straightening the rope, from the experimental results it follows that the work done per minute is approximately M $\frac{l}{R}$ where M is the bending moment required to bend the rope to this radius.

In the case of rope of 1 in. diameter consisting of six strands of seven equal wires

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BENDING STRESSES IN WIRE ROPES CONSISTING OF SIX STRANDS OF NINETEEN WIRES EACH, ANGLE OF LAY 18 DEGREES.

The Table gives the Total Pull on the Rope that would cause the same Intensity of Stress as is produced by the Bending.

Circumference of Rope.	Diameter of Rope.	Total Area of Wires.	Diameter of Drum in Feet.								
			2.	3.	4.	5.	6.	7.	8.	9.	10.
ins.	ins.	sq. ins.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
1½	·477	·090	2,776	1,851	1,388	1,110	925	793	694	617	555
1¾	·557	·123	4,430	2,954	2,215	1,772	1,477	1,266	1,107	985	886
2	·636	·161	6,621	4,414	3,310	2,649	2,207	1,892	1,655	1,471	1,324
2¼	·716	·204	9,445	6,297	4,722	3,778	3,148	2,699	2,361	2,099	1,889
2½	·795	·251	12,904	8,603	6,452	5,161	4,301	3,687	3,226	2,868	2,580
2¾	·875	·305	17,258	11,505	8,629	6,903	5,752	4,931	4,314	3,835	3,451
3	·955	·363	22,417	14,945	11,208	8,967	7,472	6,405	5,604	4,982	4,483
3¼	1·034	·425	—	18,945	14,208	11,367	9,472	8,119	7,104	6,315	5,683
3½	1·114	·494	—	23,725	17,793	14,235	11,862	10,168	8,896	7,908	7,117
3¾	1·193	·566	—	—	21,832	17,466	14,555	12,476	10,916	9,703	8,733
4	1·273	·645	—	—	26,598	21,239	17,699	15,171	13,299	11,799	10,619
4¼	1·352	·727	—	—	31,780	25,424	21,187	18,160	15,890	14,125	12,712

BENDING STRESSES IN WIRE ROPES CONSISTING OF SIX STRANDS OF SEVEN WIRES EACH, ANGLE OF LAY 18 DEGREES.

The Table gives the Total Pull on the Rope that would cause the same Intensity of Stress as is produced by the Bending.

Circumference of Rope.	Diameter of Rope.	Total Area of Wires.	Diameter of Drum in Feet.								
			3.	4.	5.	6.	7.	8.	9.	10.	11.
ins.	ins.	sq. ins.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.	lbs.
1½	·477	·092	2,876	2,157	1,725	1,438	1,232	1,078	959	812	784
1¾	·557	·126	5,043	3,782	3,026	2,521	2,161	1,891	1,681	1,513	1,375
2	·636	·164	7,495	5,621	4,497	3,747	3,212	2,810	2,498	2,248	2,044
2¼	·716	·209	10,753	8,065	6,452	5,376	4,609	4,032	3,584	3,226	2,933
2½	·795	·257	14,682	11,012	8,809	7,341	6,292	5,506	4,894	4,404	4,004
2¾	·875	·312	19,618	14,713	11,771	9,809	8,408	7,356	6,539	5,885	5,350
3	·955	·371	—	19,095	15,276	12,730	10,912	9,547	8,487	7,638	6,943
3¼	1·034	·435	—	26,396	21,117	17,597	15,083	13,198	11,732	10,558	9,598
3½	1·114	·505	—	30,320	24,256	20,213	17,326	15,160	13,476	12,128	11,025
3¾	1·193	·579	—	—	29,782	24,819	21,273	18,614	16,546	14,891	13,537
4	1·273	·659	—	—	36,171	30,142	25,836	22,606	20,095	18,085	16,441

our previous formula for M gives M = 150 in. lbs. Hence if this rope is travelling at the rate of 1,500 ft. per minute over a pulley of 9 ft. diameter, the work done per minute in bending it over the pulley amounts to 4,166 ft. lbs., equivalent to about ⅛ h.-p. Of course, this does not take into account the friction of pulley bearings.

In conclusion.—Tables are appended, computed from the formulæ given in the paper, showing the pull on ropes of various sizes that would produce the same intensity of stress as is caused by bending round drums or pulleys of various diameters. In applying the formulæ to any particular ropes it must be borne in mind that the formulæ have been established on the assumption that the wires of the rope are not strained beyond the

elastic limit. It is easy to get ridiculous results from the formulæ by applying them to ropes bent over too small pulleys, but our calculations do not apply to cases where the wires are strained to such an extent that they do not completely recover.

[Proceedings of the Australasian Institute of Mining Engineers.]

THE BRITISH ASSOCIATION MEETING AT DUBLIN.

Address to the Engineering Section.

By DUGALD CLERK, F.R.S., M.Inst.C.E., President of the Section.

At the middle of the last century the steam engine had attained to a high degree of perfection. Its development was, it is true, incomplete, but it had been successfully applied to all the great duties of the mine, the waterworks, the factory, the railway, and the steamship. The engines were mechanically excellent; the fuel economy was good, and they were built in units of thousands of horse power. Steam power, in fact, was revolutionising the whole of the social and industrial conditions of the globe. Notwithstanding this great material and engineering success, the world was in complete darkness as to the connection between steam motive power and heat. It was seen that motive power of almost any magnitude could be obtained by the agency of heat; but how it was obtained and how much power was connected with a given quantity of heat was quite unknown. The fuel consumptions of existing engines were known, and certain modes of improving economy were evident, and engineers were busily engaged in testing these modes by the slow but sure methods of invention, design, construction, and operation in practical work; but in this they had but little aid from pure science.

The science of thermodynamics did not yet exist.

New light was dawning, however, which gradually illumined the whole world of pure science and engineering practice.

THE NATURE OF HEAT.

Men of the first rank in intellect—Newton, Cavendish, Rumford, Young and Davy—had long before expressed the opinion that heat was not material in its nature, but was a mode of motion; but their opinions, although to some extent supported by experiment, made little impression upon the scientific world, and in 1850 we still find the most distinguished physicists adhering to the "caloric" or material theory of heat.

THE CARNOT CYCLE.

The great change, from the errors of the old theories to the truth of the new, was due to the work of Joule, Thomson, and Rankine in Great Britain, and of Carnot, Meyer, Clausius, Helmholtz, and Hirn on the Continent. The story begins with the work of Carnot in 1824, who published in Paris in that year a pamphlet entitled "Reflections upon the Motive Power of Heat." He was attracted by the problem of the steam engine and the air engine. He saw that heat and motive power were connected in some manner, and he endeavoured to settle in a quantitative way the limits of that connection by the invention of an ideal series of operations by means of which the greatest conceivable amount of mechanical power may be obtained from a given quantity of heat under given circumstances. For the purpose of his demonstration he assumes only two things: (1) That if heat be added to any body under standard conditions of temperature, pressure, and volume, and the body be carried through any series of mechanical processes, returning ultimately to the standard condition of temperature, pressure, and volume, then the quantity of heat added to the body is the same as that which has been discharged from it; (2) No process can exist whereby a given mechanical energy can increase its own quantity. On these indisputable assumptions he bases his ideal cycle, which consists of four simple and easily imagined operations, occurring within a cylinder behind a piston, so arranged that during the cycle work can be done by the working fluid upon the piston or work done by the piston on the working fluid.

First Operation.—The given volume of the working fluid is to be imagined as confined at its highest temperature and pressure behind the piston, and heat is to be added to keep the

temperature constant, while the fluid expands, moving the piston and doing work upon it.

Second Operation.—The supply of heat is cut off, and the working fluid expands also during work on the piston, while its temperature falls to the lowest point and its volume increases to its maximum.

Third Operation.—The piston returns, compressing the working fluid, but allowing the heat of compression to escape, so that the temperature remains during the operation at its lowest point.

Fourth Operation.—The piston compresses the working fluid, without allowing any loss of heat, to such an extent that the temperature rises again to its highest point, and the working fluid exists at the end of this operation at the same volume, pressure, and temperature as at the beginning.

This assumed series of operations would give a certain available work area, the indicated power of the engine, inasmuch as the work done by the working fluid would be greater than that done upon it. If, however, it be assumed that in all the operations the direction of motion of the piston be reversed, then compression without loss of heat would take place in the second operation; further compression, but with sufficient heat loss to keep temperature constant, would occur on the first operation; the fourth operation would follow with expansion, and the third operation would conclude also with expansion. The engine would be reversed by beginning with the second operation, moving the piston backwards in the order second, first, fourth, third. Carnot shows that this reverse operation would be performed by exactly the same amount of work as was given out by the direct operation, and that an amount of heat would be returned at the higher temperature equal to that which was added in the first case.

An engine which fulfils these conditions, Carnot states, will give the greatest amount of work which can be obtained from a given quantity of heat falling through a given temperature range. And it is evident that this must be so, because, if we assume the existence of any engine under the same conditions giving a greater amount of work from the same heat, then that engine could drive a Carnot engine in the reverse direction in such proportion as to return to the higher temperature a greater amount of heat than it abstracted, and so mechanical energy could be obtained without any heat fall whatever. This marvellous demonstration is obviously independent of the nature of the working fluid; it applies equally to all working substances, whether solid, liquid, or gaseous, whether the physical state changes or not. It at once gives a standard of the limit of mechanical power which could possibly be obtained from a given amount of heat and a given temperature fall.

The Carnot cycle operations, as here given, are applicable either to the material or to the dynamical theory of heat; but Carnot orginally stated that the whole of the heat added in the first operation was to be discharged in the third. Under the material or caloric theory, work was supposed to be done by the fact of fall in temperature. Naturally, as the heat was material it could not be destroyed or changed into mechanical energy. The production of mechanical energy was supposed to be incidental to the fall of temperature, much in the same way as mechanical energy was produced by the fall of water level, and this analogy is used throughout Carnot's work of 1824.

Carnot thus succeeded in proposing a standard of efficiency which was applicable to any heat engine, whatever the working fluid and whatever the operative cycle. By his method a limit could be set, fixing the maximum of mechanical energy to be obtained from a given heat quantity and a given temperature range. To reduce this to numerical values it was necessary, however, to experiment on any one working fluid within the desired temperature range in order to determine the work area in its relation to heat quantity and temperature fall. Carnot's writings show that he intended to make such observations; and, had he succeeded, thermodynamics would have become a science at an early date. Carnot's death, however, in 1832, at the sadly early age of thirty-six years, prevented this development.

The name of Sadi Carnot will always be remembered by mankind as the founder of one branch of the thermodynamics of the heat engine.

His work remained practically without notice for thirteen years after his death, when, fortunately, it attracted the attention of William Thomson during his attendance at the Laboratory of Regnault in the year 1845. Thomson was then twenty-one years of age, and had already attained a considerable scientific reputation. He took up the study of Carnot's work with enthusiasm. He became Professor of Natural Philosophy in the University of Glasgow in 1846, and in 1848 he read a Paper before the Cambridge Philosophical Society "On an Absolute Thermometric Scale founded on Carnot's Theory of the Motive Power of Heat and calculated from Regnault's Observations." Like Carnot, Thomson accepted the "material" or "caloric" theory of the nature of heat, although, like Carnot also, he had doubts as to its truth. Assuming its truth, however, he carried Carnot's reasoning much further, and deduced from the Carnot cycle a thermometic scale which was absolute in the sense that it defined the idea of temperature independently of the properties of any particular body.

THE ABSOLUTE TEMPERATURE SCALE.

It is very difficult to carry one's mind back to the material theory of heat, but it is necessary to do so in order to appreciate the rigid accuracy of the reasoning of both Carnot and Thomson; and it is especially desirable to do so in order to understand the great step made in this Paper.

According to the "caloric" theory, heat was supposed to be a subtle elastic fluid which permeated the pores of bodies and filled the interstices between the molecules of matter. The fundamental quality imagined of this caloric or heat fluid was that of indestructibility and uncreatability by any humanly controlled process. Bodies became warmer when caloric was added to them, and grew colder as it left them. Caloric, however, might be added to a body without heating it. In this case the heat was called "latent," and the state of the body changed from solid to liquid or from liquid to vapour or gas.

Caloric, too, was required in greater quantities for some substances than others in order to warm the body equally. The capacity for caloric was thus greater in some bodies than in others.

If any particular body were heated without change of state it was hotter; that is, its temperature rose when the quantity of caloric present was increased. It was not difficult to define equality of temperature. This was defined by a constant condition when brought into contact. But it was very difficult indeed to define temperature on any rational scale.

To the acute and brilliant intellect of William Thomson it became apparent that he had in the Carnot cycle a powerful instrument capable of widely general use, apart altogether from the theory of heat engines; and he here uses it in a most skilful way to give definiteness and universal application to the idea of temperature, as Professor Larmor states, "elevating the idea of temperature from a mere featureless record or comparison of thermometers into a general principle of physical nature."

Thomson accordingly defines equal differences of temperature in terms of the reversible or Carnot engine.

Equal temperature differences are to be differences between the temperatures of the source of heat and the refrigerator, when the proportion of work produced from a given quantity of heat is the same. Thermometers graduated in degrees calculated in this way could naturally be treated as instruments based on definite principles, independently of any properties of any particular material. The idea of temperature here was in rigid logical consistency with the "caloric" theory of heat, and it carried out completely the analogy between power derived from the same quantity of heat falling from a higher to a lower level, and resembling a fall of water in producing its effects. For equal quantities of "caloric," as of "water," temperature fall was regarded as similar to fall in space, and so an accurate idea of the nature of temperature difference is attained.

This definition, however, gave a scale greatly differing from that of mercurial, air, and other thermometers, the degrees defined by it corresponding to larger and larger intervals on the air thermometer as temperature increases. Professor Tait pointed out also that on such a scale the temperature of a body totally deprived of heat is negative-infinite.

All these difficulties do not detract from the fundamental importance of the idea here enunciated for the first time: the idea of an absolute thermometric scale theoretically applicable to all bodies—solid, liquid, and gaseous. On the "caloric" or "material" theory of heat, motive power is obtained during the letting down or fall from a higher to a lower level of a given quantity of heat. The quantity of heat does not alter in the process: it is only its relative level which alters. There is no reason, therefore, for mentally limiting the amount of mechanical energy obtainable from any given quantity of caloric, just as there is no reason for limiting the amount of mechanical energy to be mentally derived from a given weight. Any desired quantity of energy may be derived from a weight of, say, one pound, if it only be allowed to fall far enough, assuming gravity to be constant through the range.

The investigation of the work to be derived from a given quantity of heat at a given temperature is thus a matter of experiment, which can be settled by measurement of the properties of a few bodies.

THOMSON AND THE NATURE OF HEAT.

Reasoning, it is conceived, in this way, Thomson follows up his absolute thermometric scale work with an investigation entitled "Carnot's Theory of the Motive Power of Heat," described in a Paper read in 1849 before the Royal Society of Edinburgh, in which he calculates from Regnault's experiments on steam the power developed by a Carnot reversible engine when using one centigrade heat unit: that is, the heat necessary to heat one pound of water through 1° C. for temperatures from 1° to 231° C., the temperature falling in the engine in each case to 0° C.

In this Paper he asks himself two questions: (1) What is the precise nature of the thermal agency by means of which mechanical effect is to be produced without effects of any other kind? and (2) How may the amount of the thermal agency necessary for performing a given quantity of work be estimated?

Using Regnault's values for the properties of steam, he calculates the lines of compression and expansion without heat loss, the lines of compression and expansion with heat flow at the lowest temperature, and heat addition at the highest temperature, and thus arrives at the work area per heat unit let down. He tabulates these results, and shows that what he calls Carnot's function diminishes as temperature rises, using the ordinary centigrade scale. On the caloric theory the methods are rigidly logical and correct, but some inaccuracy is introduced by the necessity of that theory for the discharge of the same amount of heat at the third operation as is taken in on the first. The Paper is of great interest, however, because it

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shows clearly how fully the distinguished author realises the necessity for re-examining the standard ideas of the nature of heat. Two paragraphs make this very clear :—

"7. Since the time when Carnot thus expressed himself the necessity of a most careful examination of the entire experimental basis of the theory of heat has become more and more urgent. Especially all those assumptions depending on the idea that heat is a substance, invariable in quantity, not convertible into any other element, and incapable of being generated by any physical agency: in fact, the acknowledged principles of latent heat would require to be tested by a most searching investigation before they ought to be admitted, as they usually have been, by almost everyone who has been engaged on the subject, whether in combining the results of experimental research or in general theoretical investigations.

"8. The extremely important discoveries recently made by Mr. Joule, of Manchester, that heat is evolved in every part of a closed electric conductor moving in the neighbourhood of a magnet, and that heat is generated by the friction of fluids in motion, seem to overturn the opinion commonly held that heat cannot be generated, but only produced from a source where it has previously existed either in a sensible or in a latent condition. In the present state of science, however, no operation is known by which heat can be absorbed into a body without either elevating its temperature or becoming latent, and producing some alteration in its physical condition ; and the fundamental axiom adopted by Carnot may be considered as still the most probable basis for an investigation of the motive power of heat, although this, and with it every other branch of the theory of heat, may ultimately require to be reconstructed upon another foundation when our experimental data are more complete. On this understanding, and to avoid a repetition of doubts, I shall refer to Carnot's fundamental principle, in all that follows, as if its truth were thoroughly established."

In these two paragraphs Thomson sums up the whole situation in 1849, and promises further investigation and further attempts to deduce the nature of the connection between heat and work.

THE ABSOLUTE ZERO OF TEMPERATURE.

Assume, then, the truth of the caloric theory of heat, as Thomson does in the 1849 Paper: We have a complete theory of the heat engine, based on the Carnot cycle, accounting for efficiencies which vary with temperature differences but requiring no definite mechanical equivalent of heat; nay, antagonistic to the existence of such an equivalent. The caloric theory, as has been pointed out, is quite consistent with the theoretical possibility of obtaining an indefinitely great amount of mechanical energy from any given quantity of heat, providing the letting down or fall of level be indefinitely great.

At the time we are discussing—1850—the bare conception of the idea of an absolute zero of temperature is one which is startling in its boldness ; and it must have been difficult indeed then to imagine any definite line of proof which could be followed to establish the real existence of such a physical limit. We are so familiar with the existence of very high temperatures, vastly transcending the temperatures in which we personally exist, that we can hardly conceive a temperature limit on the ascending side; that is, we can hardly think of any given high temperature which could not under quite conceivable circumstances be exceeded. We know, for example, that any metal—say platinum—may be melted if its temperature be sufficiently increased ; that a further sufficient increase will convert the liquid metal to the gaseous state, and that the gaseous metal may be heated indefinitely while in that state. We know the behaviour and properties of many substances at high temperatures, and are aware of the strong tendency of all chemical compounds, when highly heated, to split up into the elementary bodies composing them. All this we appreciate, but we find it difficult to see how a point of temperature could be reached when it could be said: This is a physical limiting point on the ascending scale ; we may heat a substance up to this temperature, but it is impossible to conceive of any higher temperature. It is necessary here to distinguish between a conceivable limit to an ascending temperature and a practical limit under existing conditions. We may thus place limits, say, to the temperature of coal-gas and air explosions, or the temperatures possible from the electric arc : the limit with coal gas and air depending on one set of conditions, and the electric arc upon another set, such as the vapourising point of carbon, and so on. In the same way, at the middle of last century it would have been considered quite reasonable to suppose that human existence was carried on at an intermediate plane of temperature, and that temperatures might exist as low, relatively to our mean temperature, as our known furnace and combustion temperatures are high. At this time, no doubt, such an idea was quite a reasonable one.

No such limit could be proved, even by the aid of the Carnot cycle, reasoning on the material theory of heat. If we assume that heat is material, and that in some way temperature fall doing work resembles, as Carnot supposed, the fall of water doing work in passing from a higher to a lower level, then no absolute zero is possible, because the same quantity of heat is supposed to exist at the low as at the high temperature. On this theory nothing in the idea of temperature suggests a possible physical limit. On the material theory, the notion of temperature is one to which it is exceedingly difficult to attach a precise meaning.

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JOULE'S INVESTIGATIONS: THE MECHANICAL EQUIVALENT.

Thomson's promises of further investigation were fulfilled in 1850, in which year he definitely accepted the dynamical theory of heat and finally abandoned the material. His conclusions are given in a Memoir of the first importance which was read before the Royal Society of Edinburgh in 1851. It was entitled "On the Dynamical Theory of Heat." Before dealing with it, however, it is desirable to consider the work of Joule and others on another side of thermodynamics.

Long before 1850 the equivalence of mechanical work and heat quantity had been accepted by many scientific men, and Rumford had, indeed, made measurements of a rough kind. It remained, however, for Joule experimentally to determine the mechanical equivalent in the most accurate manner and place what is now known as the first law of thermodynamics upon the sure basis of absolute experimental determination. His first Paper was read before the Cork Meeting of the British Association in 1843, and at the Oxford Meeting in 1847 he read another—"On the Mechanical Equivalent of Heat"—describing the results of experiments with paddles rotating in liquids driven by falling weights. By these years of work he had absolutely demonstrated the equivalence of heat quantity and mechanical work, so that no loophole of escape seemed possible; it appeared as if the material theory was rendered intellectually impossible to the trained intellect. This was not the fact, however, as is evident from both Joule's and Thomson's accounts of that British Association Meeting.

JOULE AND THOMSON.

Joule's earlier Paper had been coolly received. Indeed, it is evident that the idea of a mechanical equivalent of heat was still distasteful to the physicists of the day, and its discussion was looked upon with dislike. Joule, at the 1847 Meeting, addressed a small audience, and the account of his experiments was received without enthusiasm. This adverse atmosphere, so discouraging to the investigator, was quickly removed, however, when a young man rose to make his remarks, and, by his enthusiastic comment and clear reasoning, at once succeeded in attracting the interest of those present. This young man was William Thomson, Professor of Natural Philosophy in the University of Glasgow. Speaking of this, his first meeting with Joule, at Manchester forty-six years later, Lord Kelvin said: "I can never forget the British Association at Oxford in the year 1847, when in one of the Sections I heard a paper read by a very unassuming young man, who betrayed no consciousness in his manner that he had a great idea to unfold. I was tremendously struck with the Paper. I had first thought it could not be true because it was different from Carnot's theory, and immediately after the reading of the Paper I had a few words of conversation with the author, James Joule, which was the beginning of our forty years' acquaintance and friendship. . . . I gained ideas which had never entered my mind before, and I thought I, too, suggested something worthy of Joule's consideration when I told him of Carnot's theory." This Meeting was indeed fateful for the future of the science of thermodynamics, as it resulted in co-operation between two men of giant intellect, who between them performed most of the experimental work which was necessary to make thermodynamics an exact science. Their work alone sufficed to place the first and second laws of thermodynamics on the firm footing of accurate experiment and logical deduction.

Although Thomson was much struck by Joule's experiments, he did not accept the dynamical theory of heat at once. As he stated himself: "I had first thought that it could not be true because it was different from Carnot's theory."

Joule's discoveries at this date may be thus expressed:—

Heat and mechanical energy are mutually convertible, and heat requires for its production, and produces by its disappearance, mechanical energy in the proportion of 1,390 ft. lbs. for each Centigrade heat unit, a heat unit being the amount of heat necessary to heat 1 lb. water through 1°C.

Knowing, as Thomson did, that mechanical energy could be produced by the agency of heat, but that its amount varied with the temperature and temperature fall, Joule's discoveries seemed antagonistic to Carnot's demonstration; and, convinced as he was that Carnot's law was true, he naturally felt at first that there must be some other way of looking at Joule's results than that adopted by Joule himself.

Joule naturally believed in his own manner of looking at his results, and he apparently agreed with Thomson as to the antagonism between what may be here called the Carnot and Joule laws.

The material theory of heat might have been true; in which case there was no more need for any direct quantitative connection between heat quantity and mechanical energy than between the mass of a body and its mechanical energy. Any unit of mass may acquire any conceivable amount of mechanical energy if its velocity be great enough, and so any unit of heat on the caloric theory may produce any conceivable amount of mechanical energy if the temperature fall be great enough. Joule considered the Carnot law to be so inconsistent with his law that in one of his Papers he proposes its abandonment as inconsistent with discovered facts. At this point the two ideas seem to be in opposition. The germ of reconciliation, however, is found in observations by Thomson in both the 1848 and 1849 Papers. In paragraph 8, quoted here from the latter Paper, it is stated:—

"In the present state of science, however, no operation is known by which heat can be

absorbed into a body without either elevating its temperature or becoming latent and producing some alteration in its physical condition."

This is equivalent to saying that no case has been observed where heat disappears doing mechanical work. In a note occurring in the same Paper he alludes to the fact that engineers always assume that the amount of heat found in the condenser of the steam engine was the same as that taken into the engine by the steam, in the following terms:—

"So generally is Carnot's principle tacitly admitted as an axiom that its application in this case has never, so far as I am aware, been questioned by practical engineers."

This was quite accurate. Hirn's demonstrations that heat disappears in a steam engine when work is done was not made until 1857, eight years later.

In the 1848 Paper he states:—

"The experiments of Mr. Joule of Manchester seem to indicate an actual conversion of mechanical effect into caloric. No experiment, however, is adduced in which the converse operation is exhibited; but it must be confessed that as yet much is involved in mystery with reference to the fundamental questions of natural philosophy."

Here we find Thomson's mind engaged—in 1848 and 1849—with the very matter requiring proof. Joule had proved the generation of heat by means of mechanical work; Thomson required the proof of the converse case—the disappearance of heat when mechanical work was done by the working fluid.

This proof was forthcoming in the results of experiments on the compression and expansion of air. Accordingly, we find the Carnot and Joule principles reconciled in Thomson's Paper of 1851, and the important deduction made of an absolute zero of temperature at -273° on the Centigrade scale. The introduction of the idea of the mechanical equivalent of heat leads at once to an absolute zero of temperature, and allows of the determination of this physical lower limit by use of the Carnot cycle for investigating the efficiency of a perfect engine using any working fluid. Air was the working fluid actually investigated, and the determination of its properties at ordinary temperatures was a vitally important result of the co-operation of Thomson and Joule. Their experiments lasted for many years, and their rigorous investigation disclosed the fact that internal work was done in expanding a gas; in fact, that in a gas expanding isothermally doing work, part of the heat only disappeared in external work and part was absorbed in separating the molecules.

The Joule and Carnot laws are now known as the first and second laws of thermodynamics.

The second law, in modern form, may be thus stated:—

Although heat and work are mutually convertible and in definite and invariable proportions, yet no conceivable heat engine is able to convert all the heat given to it into work. Apart altogether from practical limitations, a certain portion of the heat must be passed from the hot body to the cold body in order that the remainder may assume the form of mechanical energy.

The proportion of the total heat convertible into mechanical energy depends on the absolute temperatures of the hot and cold bodies; it is unity *minus* the lower absolute temperature upon the upper absolute temperature.

THE WORK OF CLAUSIUS.

It appears that during Thomson's struggle to reconcile the two apparently opposing laws, Clausius, who had seen the same difficulty, arrived independently at its solution and published a Paper, "On the Motive Power of Heat and the Laws of Heat which may be deduced therefrom," at the Berlin Academy in February, 1850. In this Paper Clausius discusses Thomson's difficulties, and also arrives at the conclusion that the Carnot cycle may be reconciled to Joule's law by the omission of the supposition that during the third process the same amount of heat is discharged from the cool body as was taken in from the hot one. He states:—

"On a nearer view of the case we find that the new theories were opposed not to the real fundamental principle of Carnot, but to the addition that no heat is lost. For it is quite possible that in the production of work both may take place at the same time; a certain portion of heat may be consumed and a further portion transmitted from a warm body to a cold one; and both portions may stand in a certain definite relation to the quantity of work produced. This will be made plainer as we proceed; and it will be moreover shown that the inference to be drawn from both assumptions may not only exist together, but that they may mutually support each other."

In his 1851 Paper, Thomson gives Clausius full credit for solving the difficulty between the Carnot and the Joule principles. Thomson gives Clausius the full credit for priority, but states that he was working on the same problem and had arrived at the same solution in the year 1850, before he had seen Clausius' work. Clausius, however, assumed the theory of a permanent gas, which required the absence of internal work, but Thomson was not prepared to assume this without experiment. This determination rigidly to prove every necessary assumption, and his clear conception of the points necessary for proof, led to the extensive series of researches undertaken by Thomson and Joule with the object of determining how much gas thermometers differ from an absolute scale as determined by the combination of the Joule and Carnot laws.

RANKINE'S EQUATION.

Rankine, as early as 1849, arrived at the general equation of thermodynamics which expresses the relation between heat and

mechanical energy, and indicated the result of his investigations to the Royal Society of Edinburgh, in February, 1850. Rankine thus arrived independently at the same result as Clausius about the same time. Both Rankine and Clausius, however, adopted certain theories as to the molecular structures and motions of gases, and their demonstrations to some extent depended upon their theories. To Thomson and Joule we are deeply indebted for the rigid proof of the two laws and for the rigid deduction of the modern scale of temperature and the determination of absolute zero in its modern form. Thomson now thus defines temperature :—

"The temperatures of two bodies are proportional to the quantities of heat respectively taken in and given out in localities at one temperature and at the other respectively, by a material system subjected to a complete cycle of perfectly reversible thermodynamic operations, and not allowed to part with or take in any other temperature ; or, the absolute values of two temperatures are to one another in the proportion of the heat taken in to the heat rejected in a perfect thermodynamic engine, working with a source and refrigerator at the higher and lower of the temperatures respectively."

This definition leads to an absolute scale of temperature which is independent of the substance operated on, and Joule and Thomson's experiments have shown that this scale differs but slightly from that of the ordinary air thermometer. Joule had suggested to Thomson, in a letter to him in 1848, that the probable value of Carnot's function is the reciprocal of the absolute temperature as measured on a perfect gas thermometer.

Thus Clausius appears to have anticipated Thomson, not in the suggestion of an absolute scale of temperature, but in the idea of an absolute zero founded upon the combination of Carnot's law and Joule's law. Thomson, in his Papers, very modestly attributes the second law—the law of the transformation of heat—to Carnot and Clausius ; but in this he undervalued his work, because Clausius appears to have assumed what Thomson and Joule proved : that is, the coincidence of the absolute scale with the air thermometer scale.

It will thus be seen that the position usually assumed by the engineer at 1850, of the equality between heat given to the engine and heat given to the condenser, was fundamentally untrue. Without this deduction, however, no determination of the values of the Carnot function could have led to the determination of an absolute zero. According to the material theory, as seen in the light of Carnot's cycle, a heat unit could give an indefinitely increased amount of work with lowering of the temperature. Nothing in the theory set a limit to this increase, and, accordingly, there is nothing to suggest an absolute zero. Immediately, however, we accept the dynamical theory of heat we find that a pound of water requires the exertion of 1,390 ft. lbs. of work to heat it through 1° C. We also know from the Carnot cycle that under ordinary conditions of human existence only a portion of this work can be returned : but as no conditions could conceivably exist in which a greater amount of work could be obtained from a pound of water than the 1,390 ft. lbs. put into it to heat it through 1° C., it follows that, inasmuch as the Carnot function increases with diminishing temperature, the limit of temperature is reached when, according to the Carnot cycle, the whole of that work, put into the pound of water, can be got out again as work. This limit is the absolute zero of temperature. No lower temperature is conceivable without introducing the idea of the creation of energy. So far as human beings are concerned, this idea is as inconceivable as the idea of the creation of matter. The determination of this limit with the close accuracy necessary for a well-founded constant is to be entirely attributed to Thomson and Joule. In his 1851 Paper Thomson thus succeeds in answering the questions which he put to himself in his 1849 Paper, and he supplies a quantitative method of connecting the amount of the thermal agency necessary with the amount of work which can be performed under varying conditions.

Engineers dealing with motive power are thus deeply in debt to Thomson and Joule for the secure position occupied by them to-day.

The brilliant work of Meyer, published so early as 1842, is held by some to have anticipated to a large extent both the work of Thomson and of Joule. Undoubtedly Meyer formulated true ideas and carried his generalisations through a wide range. Helmholtz also very early arrived at similar conclusions to those of Joule and Thomson : but it has been thought better to discuss the work of Thomson and Joule separately, in order to illustrate the transition period through which many distinguished minds were passing about the time. Undoubtedly great credit is due to Meyer, Helmholtz, Clausius, and Hirn, and Thomson himself recognised this in the most generous way.

THE POSITION OF THE HEAT ENGINE IN 1850.

The ideas of Thomson and Joule now form so much of the basis of all reasoning upon motive-power engines that there is some little danger to the present generation of forgetting what they owe to these two great men. To appreciate the step made by them it is necessary to consider the position of motive power produced by heat at about the middle of the last century. At that time many attempts had been made to displace the steam engine as a heat engine by air engines in various forms—both engines heated externally and those heated internally, now known as internal-combustion engines. Papers read at the Institution of Civil Engineers in 1845 and 1853, and the discussion of those Papers by eminent men of the day, supply an

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accurate measure of the knowledge possessed by the engineer of the principles of action of his heat engines. Many distinguished names occur in these Papers and Discussions, including James Stirling, Robert Stephenson, Sir George Cayley, Charles Manby, James Leslie, C. W. Siemens, Hawksley, Pole, W. G. Armstrong (afterwards Lord Armstrong), Edward Woods, E. A. Cowper, D. K. Clark, Benjamin Cheverton, Goldsworthy Gurney, George P. Bidder, Professor Faraday, Isambard K. Brunel, Captain Fitzroy, and F. Braithwaite. At the date of the later of these discussions Brunel had already designed the "Great Eastern," 1852, with its engines of 11,000 h.p. Armstrong was a Fellow of the Royal Society, and had started the Elswick Works and invented the Armstrong gun. Robert Stephenson was at the height of his fame. He was then a Member of Parliament, President of the Institution of Civil Engineers, and a Fellow of the Royal Society. Siemens was a young man, but was busy on the regenerative furnace; had considered regeneration as applied to steam engines, although his work on the air engine was still to come. All were distinguished men in their day, and their opinions may be taken as representing the very best scientific knowledge of the leading engineers of the day.

THE HOT AIR ENGINE.

The first of the Papers to which I refer is called "Description of Stirling's Improved Air Engine," by James Stirling, M.Inst.C.E. It was read on June 10th, 1845, with Sir John Rennie, the President of the Institution, in the chair. The engine described was the later form of the well-known Stirling air engine, invented by the Rev. Dr. Stirling, a Scottish clergyman, in the year 1815. The development considered was the invention of the reader of the Paper, a brother of Dr. Stirling. The main improvement consisted in the use of air at a greater density than the atmosphere, and the engine at that date had so far succeeded that two had been used at the Dundee Foundry Company's works—one giving about 21 h.p. and the other about 45 h.p. Practically, therefore, some success had been attained. Mr. Stirling claimed that the 21-horse engine consumed 50 lb. of coal per hour, which is about 2½ lbs. per h.p. per hour. This was an extraordinarily good result for the time. At present, however, we are not interested in the practical result, but only in the opinions of the engineers of the day as to the fundamental principles of heat engines.

It is clear from the Paper that the theory of the regenerator was entirely misunderstood. It was imagined that with a perfect regenerator no heat would be required to perform work. This is evident from Mr. Stirling's answer to Sir George Cayley. Sir George Cayley described his engine, which was of the internal-combustion type, acting with solid fuel under constant pressure, and showed that, owing to dust and heat in the cylinder and valves, his experiments proved abortive. He stated, however, that his engine had consumed 6¾ lbs. of coke—equal to 9 lbs. of coal—per h.p. To this Mr. Stirling answered: "It must be remarked that Sir George Cayley, in following an entirely different object had overlooked the great leading principle of repeatedly using the same heat," and "he was of opinion that, except on that principle, the air could not be economically used as a moving power." Another speaker, Mr. Cottam, said: "It was evident that, if it was practicable to arrive at the theoretical condition of the absorption of all the caloric by the thin laminæ during the upward passage of the air and the giving out again during the downward passage, there would not be any loss of heat." Mr. Robert Stephenson did not appear to understand Stirling's air engine at all, because he made the following remarks: "He understood the process to consist of heating the air in a vessel, whence it ascended to the cylinder between numerous thin laminæ, by which the caloric was absorbed, to be again given out to the descending air. Now it appeared to him that, though the ascending process was natural and easy, the reverse action would require a certain expenditure of power, in the depression of the plunger." This remark clearly showed that Stephenson, notwithstanding his eminence as an engineer, at that date had not appreciated the essential conditions of the hot-air engine.

In the year 1853 the subject of the air engine again came up before the Institution of Civil Engineers, interest being excited evidently by the building of the large engines of the hot-air ship *Ericsson* in America, the engines having air cylinders of no less than 14 ft. diameter. Four Papers were read in this year: "On the Use of Heated Air as a Motive Power," by Benjamin Cheverton; "On the Caloric Engine," by Charles Manby; "On the Principle of the Caloric Air Heated Engine," by James Leslie, M.Inst.C.E.; and "On the Conversion of Heat into Mechanical Effect," by Charles William Siemens, A.M.I.C.E.

Cheverton evidently considers, from his Paper referring to Stirling and Ericsson, that "Both parties also rest the efficiency of their engines on the repeated use of caloric. They contend that in recovering from the ejected hot air the caloric which gave it superior tension, and employing it in heating the injected air, 'it is made to operate over and over again.' Mr. Ericsson aspires to embody a new principle in motive mechanics—no less, to use his own words, than 'that the production of mechanical force by heat is unaccompanied by the loss of heat,' except such as arises from radiation, or other practically unavoidable waste." Cheverton rejects this idea, but, strangely enough, does not appear aware of the work either of Carnot or of Joule. He comes to the conclusion, however, that "caloric, doubtless, is in all its aspects a manifestation of force, and, unquestionably, as a mechanical agent, of a dynamic force, and therefore is directly amenable to the third law of motion." He appears

to think that heat is accompanied with molecular activity, but is puzzled by what he accepts to be a fact, that in the steam engine the whole of the heat of the steam as it comes from the boiler is found in the condenser. With regard to the steam, he says: "Undoubtedly, in respect to the materiality of caloric, if it be material, it is transferred intact to the condenser, yet in its passage it may have parted with force, which it cannot communicate again." He comes to the conclusion that the change may take place not in the quantity, but in the intensity of heat. Here he resembles Carnot; but it appears to him impossible to arrive at any useful theory of the heat engine, because he states: ". . . for every investigation leads to the conclusion that the effect of caloric is independent at least of the chemical, if not also of the physical, constitution of bodies. But economy of fuel is a different question from the economy of caloric; it is altogether a practical matter, and can only be determined by experiment; for this, and, indeed, most other points of practice, are too intractable to come within the grasp of the most powerful calculus." In the discussion a communication was read from Sir George Cayley, in the course of which he states, with regard to the regenerator: "There can exist no doubt of the effective re-application of heat to an almost unlimited extent by this beautiful invention, due originally to Mr. Stirling, and now carried out to a greater extent by Captain Ericsson." Sir George Cayley discussed the difficulties of Ericsson's engine, but he accepts the principle that heat may give work and yet be used over and over again practically undiminished. Armstrong did not express himself upon the theory at all, but he was doubtful as to the advantage of the air engine compared with the steam engine, although he believed that it was practicable to recover and use over again a large proportion of the heat applied, and he thought the balance of economy, so far as heat was concerned, would be found in favour of air. Siemens agreed to some extent in the advantages of a regenerator, but he showed clearly that expansion doing work was accompanied by a diminution of temperature, and stated that this heat had to be replaced by the fire. Bidder was of opinion "that no theoretical advantage was obtained in using heated air instead of vaporised water as a motive power, and it was incapable of being applied practically with as much convenience." It is most interesting to note that Dr. Faraday joined in this discussion. He said very little, and I will give his remarks complete. Dr. Faraday said: "Twenty years ago he had directed his attention to this question, and from theoretical views he had been induced to hope for the successful employment of heated air as a motive power: but even then he saw enough to discourage his sanguine expectation, and he had, with some diffidence, ventured to express his conviction of the almost unconquerable practical difficulties surrounding the case, and of the fallacy of the presumed advantages of the regenerator. He still retained his doubts as to the success of the innovation, and feared the eventual results, even of Captain Ericsson's spirited and ingenious efforts." Brunel considered the use of the regenerator to be an entire fallacy, and did not believe that the power derived from the expansion of air by heat could be used effectively, and then be recovered and used again. Mr. Hawksley considered that the machine involved a mechanical fallacy, and that the regenerator produced no mechanical effect whatever. Mr. Rendel was the President at the Meeting which dealt with Mr. Cheverton's Paper, and, in view of the great differences of opinion on the subject, he stated that "he would not have the Meeting arrive at a hasty or erroneous conclusion on the question of this engine, and he therefore suggested that Mr. Siemens should draw up a Paper on the subject, and that the Members should collect, for a future Meeting, all the information within their reach, in order to the calm and deliberate discussion of the question." This resulted in the further Meeting of May 17th, 1853, when Papers were read by Mr. Manby, Leslie, and Siemens. The Paper by Manby consists of the summary of a discussion by M. Galy-Cazalet, which took place in Paris in 1852. M. Galy-Cazalet comes to the conclusion that the regenerator involves a fallacy, and he concludes: "There appears to be at present so much doubt of the utility of the regenerator that it would be wise to abandon its use for a time, and by trials with a more simple form of caloric engine establish the fact either of the superiority or of the inferiority of heated air in comparison with steam as a motive power." Mr. Leslie, on the contrary, in his Paper upholds vigorously the accuracy of the principle of the regenerator or economiser. He comes to the conclusion that it is based on true principles and is attended in practice with real economy of heat, and consequently of fuel. In this conclusion he is doubtless correct; the regenerator is useful and does economise heat. But Leslie goes much further than this: he appears to support Stirling in the fallacy that the regenerator may be made indefinitely useful. Stirling states:—

"And thus it appears that by applying air successively to a series of bodies regularly increasing in temperature, and moving it alternately from one end of the series to the other, it may be heated and cooled ten times, with an expenditure of caloric which would barely have heated it once, if it had been applied at once, to the hottest body (*i.e.* beyond the series). It is evident also that if the series had been composed of twenty points, or bodies, having a difference of temperature of five degrees, the air might be heated and cooled twenty times at no greater expense of caloric. Nay, it is evident that by multiplying the members of the series indefinitely air could be heated and expanded and made to do work at no appreciable expense. But let no mathematician be alarmed with the idea of a perpetual motion, or the creation of power. There are many enemies to contend

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with in the air engine besides friction, which alone prevents perpetuity in some mechanical motions. We have no means, without consuming a part of our power, of applying the air so closely to the apparatus as to make it absolutely assume the temperature of the bodies to which it is applied. There is, therefore, a loss in the very act of heating and cooling."

Leslie comes to the conclusion that Stirling is right, but that an air engine without a regenerator would be a much less effective and economical application of heat than the steam engine. Leslie gives some interesting particulars of the later air engines of James Stirling. He states that an engine of 45 h.p. was started in March, 1843, at the Dundee Foundry; that in December, 1845—two years and nine months after starting—one air-vessel gave way, and in May, 1846, another failed, and in January, 1847, a third failed. This information was supplied to him by Mr. David Mudie, one of the lessees of the foundry.

SIEMENS AND THE DYNAMICAL THEORY OF HEAT.

We now come to Siemens' Paper "On the Conversion of Heat into Mechanical Effect," and for the first time we find the engineer guided by an intelligible principle. Siemens discussed the material theory of heat, and accepted unreservedly the dynamical theory, for which he gives a large measure of credit to Joule. This is the first of the Institution Papers in which I find the name of Joule. Siemens mentions Carnot, Clapeyron, Holtzman of Mannheim, Joule, Helmholtz, Meyer, Rankine, and Professor Thomson. Curiously enough, although Siemens mentions Carnot and the other philosophers who dealt with the Carnot principle, including Thomson, he does not appear at this date—May 17th, 1853—to have realised himself the effect of the law of Carnot upon the theory of the heat engine. He clearly appreciated the first law, and gives the mechanical equivalent of heat as determined by Joule at 770 ft. lbs., and by Thomson's formula as 773 ft. lbs., but in his discussion of the principles of the heat engine he is of opinion that a perfect engine is ideally possible giving 770 ft. lbs., for each Fahrenheit heat-unit employed. This is clear from a Table found on page 33 of the Paper, which I reproduce below.

He apprehends the mechanical equivalent of heat, but he still appears under the impression that if heat be added to a certain upper temperature and expansion take place until the original temperature is reached, then he has a perfect engine indicating the full result of Joule's mechanical equivalent. He sees, however, that the old theory of the regenerator is quite wrong. He states:—

"The cause of the failure of Mr. Stirling's engine in practice may apparently be traced chiefly to insufficiency of heating surface, occasioned apparently from misapprehension

SIEMENS TABLE OF 1853.

Description of Engine.	Theoretical performance in ft. lbs.	Actual performance in ft. lbs.	Actual performance in lbs. of coal per h.p. per hour.
A Boulton and Watt condensing engine, low pressure	51·8	29	8·00
The best Cornish engine	158·8	82	2·38
Combined steam and expansive ether engine ...	150·0	75	3·09
The expansive air engine	91·0	45	6·63
Stirling's engine	130·0	65	3·57
Ericsson's engine	196·0	65	3·57
A perfect engine	770·0	385	0·60

of the principle involved, it having been thought that the same heat would serve over and over again to produce power, and that the necessary expenditure of heat consisted only in the mechanical loss by imperfect action of the respirative plates, which were approached to each other to the utmost limits, consistent with an unobstructed passage of the air. By the aid of the dynamical theory of heat it has been shown that there is another and far more important expenditure of heat, which should have been provided for."

Siemens, in the discussion, rightly upheld the regenerator as useful, and saw that there were limitations to its use. Mr. Hawksley contended that the regenerator was useless. Mr. Pole considered that the regenerator was useful, but he did not definitely adopt the mechanical theory of heat. He stated:—

"It must be allowed that the general action of caloric in producing power was still involved in much obscurity. The heat was often considered in reference to its quantity only, but it was certain also that its intensity performed a very important part; and it had even been surmised that power might be obtained by the reduction of intensity alone, without any change of quantity."

Armstrong concurred with Siemens and Pole. He believed in the utility of the regenerator, limited as described by both. Mr. Edward Woods certainly understood Siemens to have given 772 ft. lbs. as the efficiency of an ideal heat engine, because he stated that this showed there was still great room for improvement in engines. Mr. E. A. Cowper had clear ideas: he said:—

"Steam, or gases, in expanding, and so giving out power, lost heat. Part of the sensible heat became latent in the production of power, and this heat could only be recovered by expending the power already produced in again condensing the steam back to its original bulk, when the latent heat again became sensible.'

This discussion, then, puts us in the position of engineers at the date of the last Meeting referred to—May 17th, 1853. Of all the distinguished engineers who spoke, Siemens alone

had thoroughly apprehended the value of Joule's results and understood the full bearing of the mechanical equivalent of heat. He had not, however, understood Carnot s reasoning on the Carnot cycle, or Thomson's deductions from Carnot. He was under the impression that heat added in any way to a working fluid, raising the temperature, could be entirely converted into work by a sufficient expansion. He had not appreciated that, even if expansion be carried far enough to reduce the temperature to the original temperature before heat addition, yet complete conversion of the entire mechanical equivalent was impossible. When so able a man as Siemens had at this stage only reached partial enlightenment, it was evident that much hard work and clear thinking required to be done before a well-founded theory of heat motive-power could be obtained.

REGNAULT'S INVESTIGATIONS.

The data for such a theory was accumulating ; and one of the most interesting circumstances connected with these Institution of Civil Engineers Papers was a communication from M. Regnault to Colonel Sabine, Treasurer of the Royal Society, dated April, 1853, which was read at the Meeting, in which Regnault stated that—

"He was about to publish immediately a series of elaborate experimental researches on various subjects connected with the effects of heat on elastic fluids, the results of which would solve many questions long in dispute, and by means of which engineers might accurately calculate the effect of a given amount of fuel, in whatever way it was applied. M. Regnault communicated in anticipation that he had arrived at the number 0·237 for the specific heat of air at constant pressure, and at 0·475 for that of steam under atmospheric elasticity, the specific heat of water being taken in each case as unity."

True to his word, Regnault produced his admirable investigations, and succeeded in solving many problems ; but he did not settle the questions to the extent he had hoped. Even at the present time doubt arises as to the very values he gave for the specific heat of air and steam. The problem proved much more difficult than he had anticipated. and for modern engine purposes it cannot be considered as wholly solved now—fifty-five years later.

This description of the position of the hot-air engine, as shown by the opinions of eminent engineers, is most useful as proving how much practical men were in need of the work of Thomson and Joule. It is not surprising that, of all the engineers present, Siemens appeared to be alone in thoroughly grasping the new ideas. Thomson's own conversion from the material theory of heat to the dynamical theory was not complete until 1851, and although he had then succeeded in reconciling the ideas of Joule and Carnot, it is not to be wondered at that engineers two years later had not quite succeeded in grasping the combination of the two laws. This combination, however, supplied engineers with a new and accurate standard of measurement for studying and improving upon their heat engines, and they were by no means slow in grasping the help thus offered them by the abstract scientific man. The broad laws of thermodynamics have placed the theory of the heat engine in a position of certainty, which was much needed. It would be a mistake to assume, however, that even the determination of the mechanical equivalent of heat and the second law of thermodynamics expressed in terms of an absolute thermometric scale had solved all the difficulties of the engineer desiring to determine the efficiency of his heat engines. Thomson, Joule, Rankine, and their great Continental colleagues, it is true, settled once and for all the broad laws of thermodynamics, but the Carnot cycle is a cycle which is, as has been repeated shown, an impossible one in practice. Accordingly actual engines have to operate upon imperfect cycles.

THE IMPERFECT CYCLE.

The theory of these imperfect cycles has been worked out mostly during the last twenty-five years, although Rankine made a beginning in dealing with the theory of the Joule air engine. For the first time he showed the existence of what may be termed a cycle of constant efficiency in the case of the Joule air engine. Assuming constant specific heat for the working fluid, he calculates the efficiency of what we now call a constant-pressure air engine between certain limits of temperature, and he gives the efficiency of the fluid where U = energy exerted and H_1 = heat received, and r = ratio of compression and expansion :—

$$\frac{U}{H_1} = 1 - \frac{1}{r^{0.408}};$$

that is, he indicates in this formula that the thermal efficiency is independent of the maximum temperature as long as that maximum temperature exceeds the temperature of adiabatic compression. He makes no statement, however, that this engine is within a certain range independent of the maximum temperature : that is, that increasing maximum temperature does not increase efficiency. Subsequent work has shown that, on a simple assumption, such as constant specific heat, many engine cycles exist of a practical nature having high theoretical efficiencies where the theoretical efficiency depends on one thing only—the ratio of compression. Some misunderstanding has arisen with regard to these imperfect cycles, and it has even been thought that such imperfect cycles would be contrary to the second law of thermodynamics. Lord Kelvin himself was of this opinion in 1881. I vividly remember a conversation I had with him at the Crown Iron Works, in Glasgow, over the results I had obtained from one of my early gas engines. I had then come to the conclusion that the

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"Otto" cycle as ordinarily operated was a cycle of constant efficiency, and I explained this to Lord Kelvin. He had not followed such cycles, and his view then was that no such cycle could exist, because he thought it was contrary to the second law of thermodynamics. Some idea of this kind has been held by many scientific men, and has prevented the minute investigation of imperfect cycles of different kinds, because of the feeling that the whole question of efficiency was entirely settled by the nature of the temperature limits; that is, by the maximum and minimum temperatures at the disposal of the engineer. It is true that these values, as has been shown, must always determine the extreme limit of possible efficiencies between certain temperatures, and in cycles of constant efficiency the particular efficiency of the cycle is always less than the efficiency of a Carnot cycle engine working between the same limits of superior and inferior temperature. The investigation, however, of these imperfect cycles is much more difficult than the broad investigation of the general thermodynamic laws, because it requires accurate knowledge of the properties of the working fluid dealt with under conditions rendering observation extremely difficult.

THE INTERNAL COMBUSTION MOTOR.

The modern internal-combustion motor is the successor to the air engine so fully discussed by eminent engineers of fifty-five years ago; and the forebodings of even so eminent a man as Faraday as to its ultimate success have proved unfounded. Great difficulties have been encountered and many discrepancies have had to be explained, but a minute study of the nature of the working fluid has rendered it more and more possible to calculate the efficiencies to be expected under practical conditions. At the present time we can deal with almost any cycle or any working fluid with some fair approximation to an accurate result. Much work, however, is required before all problems of the working fluid can be said to be solved with regard to any heat engine. Indeed, it may be said that under modern conditions of the use of steam even the properties of the working fluid—steam—have not yet been satisfactorily determined. The mere question of specific heat, for example, of steam and its variations of temperature and pressure is now under review, and important experiments are in progress in Britain and on the Continent to determine those properties. The properties of the working fluid of the internal-combustion motor are also the subject of earnest study by many Continental and British investigators. Notwithstanding all the perplexities involved in the minute study of the imperfect heat engine cycles, we are in a very different position to-day compared with the engineer of 1853. We know all the broad laws as to the conversion of heat into work or of work into heat: and, numerous as are the problems yet to be solved, we at least profit by the guiding light set out for us by Kelvin, Joule, and Rankine.

Laws of Flight: Theory of Sustentation.—*By F. W. Lanchester.* The law of the pressure reaction for speeds such as may be usefully employed in aerial flight has been established experimentally, and is:—

(1) *On a body of stated geometrical form in given presentation the pressure reaction varies approximately as the square of the velocity, and as the square of the linear dimension.*

The theoretical justification of this law is that the viscosity and elasticity of the fluid as quantities of which the resistance is a function, are without sensible influence. This is approximately true, first concerning vicosity, so long as the product of the linear size and velocity of the body is greater than a certain minimum value: and secondly as to elasticity, provided that the velocity does not approach too nearly the velocity of sound. In aerial flight as practised by birds or man neither of these limitations apply so that the V-square law may be accepted as applicable. There is some correction required in regard to *skin-friction*; in resistance of this kind it would appear that the index is somewhat less than 2, though how much at present we do not know.

In the relations of the pressure reaction on *planes at small angles* there is a law of great utility to be deduced from experiment, namely,

(2) *For a given velocity the pressure varies directly as the angle.*

This law only applies to the *small angle, i.e.*, to one which, expressed in radians, the angle, its sine, and its tangent, are sensibly equal to one another. The law further does not apply to planes in *apteroid aspect*, in the extreme case of which the sine-square law of Newton can be shown to apply: neither of these limitations seriously affects the validity of the law in relation to aerial flight.

A theory founded on the *hypothesis of constant sweep*—that is, upon the assumption of a layer of air of defined thickness uniformly handled by the aeroplane—gives results in agreement with these two laws, but with a defect in the constant by which the quantities are related. This defect is accounted for and the theory is rectified by taking into account the cyclic component in the periptery: by this extension of the initial hypothesis harmony is established between theory and experiment.

The two laws so far established result in the fact discovered by Wenham and rediscovered by Langley, that neglecting skin-friction and other direct resistance the power expenditure decreases when the velocity is increased, and the law of frictionless flight is established.

(3) *Neglecting skin-friction and other direct resistance, the h.p. varies inversely as* V; *or the resistance to flight varies inversely as* V^2.

The modifying influence of skin-friction and of other resistances varying *directly* as V^2 results in the following laws, the proofs of which are

given in the author's "Aerial Flight," vol. i., *Aerodynamics*.

(4) *The total resistance to flight is least when the resistance due to aerodynamic support* $\left(\propto \frac{1}{V^2}\right)$ *is equal to the direct resistance* ($\propto V^2$); *hence this is the condition of greatest range on given fuel-supply.*

(5) *The flight will be sustained for the longest time on a given supply of energy when the resistance due to aerodynamic support is three times the direct resistance.*

(6) *The speed of greatest range is* $\frac{1}{\sqrt[4]{3}}$ ($= 1{\cdot}315$) *times the speed of least power.*

(7) *Neglecting "body resistance" for aerodones designed for least resistance, the resistance is independent of the velocity of flight.* In other words, *the gliding angle is constant, or the power varies directly as the velocity of flight.*

Cor. *When body resistance is present the total resistance consists of two parts, one of which varies as the velocity squared and the other of which is constant.*

(8) That, consequent on laws 4 and 5, there are best values of angle β (the angle of an aeroplane to the line of flight), or the angle of trail of a pterygoid aerfoil, and of the P/V^2 relation, that correspond to the condition of least resistance. These, tabulated from theory, are found to be in harmony with experience.

The Theory of Stability.

An aerodone or aerodrome may be constructed to possess automatic stability without any *special mechanism* for the maintenance of equilibrium. The natural flight path is of undulating form, the varieties of flight path obtainable being demonstrated both by theory and experiment, plottings of these curves being given.

An extension of the theory gives an equation, termed by the author the "*equation of stability*," and it is shown experimentally that this equation gives the limiting proportions of an aerodone of stable flight path.

A final consequence of the theory of stability gives the important fact that for any given design of aerodone or flying machine there is a minimum velocity of stable flight, and that the larger the machine the higher this minimum velocity becomes. For a man-bearing machine it is probably over 30 miles per hour, and in order that a sufficient margin of automatic stability should be provided, the flight velocity should be but little short of 40 m.h.

The Utilisation of Peat for making Gas or Charcoal with Recovery of By-Products.—*By* Capt. H. RIALL SANKEY, *R.E. (ret.), M.Inst.C.E.* The much discussed subject of the utilisation of peat has during the last few years been revived owing to developments in gas producers and in gas engines, and at the moment it is of considerable interest in Ireland in connection with the Bill which has been promoted in Parliament to obtain powers to produce gas from peat, to use this gas for making electricity by means of gas engines and dynamos, and to distribute electric power to works which will probably be established in the immediate neighbourhood of the power station and throughout a certain prescribed district. It is proposed to put the power station alongside the Grand Canal, not far from Robertstown, about twenty-five miles from Dublin.

Previous attempts to utilise peat for power failed because they were based on drying the peat so as to contain no more than 25 per cent. of water, and in some cases the expense of "briquetting" was incurred; the peat was then transported to the place where power was required and burnt in steam boilers fitted with specially designed furnaces. Such peat could not compete with coal: moreover, the valuable by-products were not recovered.

In the proposed scheme the peat will only be partially dried—that is, will still contain 60 per cent. of water: it will be used on the spot to make gas, so as to get the benefit of the great thermal efficiency of gas engines and to save the cost of carriage. The by-products will be recovered, the profit on which will at least cover the cost of getting and drying the peat. It will be possible to supply power to works in the immediate neighbourhood of the power station as cheaply as can be done from water power. Many of those industries which are dependent upon cheap power will undoubtedly be attracted.

Great progress has been made in Germany in the utilisation of peat. Although no complete electric-power undertaking is at present working on the above lines, all the various links in the chain are separately and successfully in operation: many of these links have been "made in Germany," why should not the complete chain be put together in Ireland?

There are several methods of getting and drying peat. Apparently, the most suitable, when it is desired to obtain peat nearly all the year round, is to dig with a grab, and pass the peat through a Dornberg press. The peat then rapidly dries to 60 per cent., except in very wet weather in the winter, and to tide over this period a reserve of peat must be accumulated.

The producers used for making gas from peat are similar to those used with bituminous coal, but they are larger in dimensions for the same power. The gas issuing from the producers passes through the recovery plant, thence to the gas engines. The gas in passing through the recovery plant is cleaned in a very perfect manner, and therefore troubles due to tar and dust are entirely obviated.

Sulphate of ammonia is the principal by-product. The amount obtained depends on the percentage of nitrogen in the peat. The proposed power station at Robertstown will be capable of making 3,000 tons of sulphate per annum.

The other important by-products are acetate of lime, methyl alcohol, and tar containing paraffin wax and oils. An excellent waggon grease

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can be made from the tar, the output of which at Robertstown ought to be about 2,000 barrels per annum. The monetary value of these by-products will be about equal to that of the sulphate of ammonia.

An excellent charcoal can be made from peat, instead of gas, together with the by-products enumerated above, and the process has been in operation near Oldenberg for over ten years. The most up-to-date factory, however, is at Buerberg, near Munich, and it has been at work for three years. The charcoal obtained from peat is of excellent quality, and can be used to advantage to replace wood charcoal. It is used in large quantities in Germany in connection with the manufacture of steel.

Producer Gas.—*By* J. EMERSON DOWSON, *M.Inst.C.E.*—The author's first paper on this subject was read at the York Meeting in 1881, and after twenty-seven years he reviewed briefly the progress since made. In large furnace work he traced the developments and improvements in the system introduced by the brothers Siemens. Its success has been great, and has been due chiefly to heating the air for combustion by the waste products. Dr. Mond's recovery of ammonium sulphate, which reduces the cost of fuel, is also mentioned. For smaller heating work, where jets of gas are required, it is essential that the gas should be clean, of good quality, and of uniform pressure. This is especially necessary where the gas must be burnt with air at pressure, as for blow-pipes, &c. Producer gas is now used for the following as well as many other purposes:—Cooking and baking in hospitals and asylums, japanning and enamelling, typefounding, varnish making, melting in crucibles, cutting and finishing glass, heating tailors' irons in clothing factories, and laundry irons, calenders, &c., grassing silk and cotton yarns, singeing textile fabrics, soldering biscuit and condensed-milk tins, &c., annealing, hardening and tempering, boiling sugar, roasting coffee, cocoa, and other food products in factories. Notwithstanding this the author considers that, in proportion to the immense amount of heating required in the various industries throughout the kingdom, more progress would have been made if manufacturers and others knew more about the subject technically. Often they do not seem to understand the treatment of gas, or what can be done with it. Undoubtedly the greatest development in the use of producer gas has been with gas engines. A gas engine was worked for the first time with producer gas in 1879, in a plant devised by the author. The calorific power of producer gas is the same now as then, but less gas is now consumed per h.p., owing to improvements made in the engines. The meaning of *pressure* and *suction* gas was described. In 1862 Dr. Jacques Arbos of Barcelona patented a gas producer worked by the suction of an engine, but the first to deal with this in a practical way was M. Léon Bénier of Paris (1891). Various modifications and improvements have since been made, and the suction plant is extensively used. The chemical reactions on which pressure and suction gases depend are identical, but the calorific power of pressure gas made with a jet of superheated steam is usually considered higher than that of suction gas. For heating purposes in small burners, or in blowpipes, pressure gas is better, and an engine worked by suction gas develops a rather lower maximum power than with pressure gas. In considering the two types of plant for engine work, the general conclusions are:—A suction plant costs less and occupies less ground space, but the gas made in it is not so strong as in the older form of pressure plant, and in some cases this is important. The fuel consumption per h.p.-hour and the labour required are the same in both types of plant, provided the steam required for the pressure gas is raised without an independent boiler. The consumption of water is the same in both types. Where there are several engines to serve, the gas piping is simplified and its cost reduced when the gas is taken from a small gasholder, instead of from several suction plants. In some cases the pressure type is better than the suction, in others suction is better than pressure. From the economical point of view, where producer gas is used for heating work instead of ordinary town gas, there is usually a saving of 50 to 60 per cent. With engines the consumption of anthracite is guaranteed not to exceed 1 lb. per b.h.p.-hour (under full or three-quarter loads) in both types of plant. Gas coke is also used, but its consumption is a little higher. For large furnace work non-caking bituminous coal is almost invariably used, and in some cases this kind of coal is also used for engine work.

The Strength of Solid Cylindrical Round-ended Columns.—*By* Professor W. E. LILLEY, *M.A., D.Sc.*—In some previous papers* by the author the importance of secondary flexure and its influence on the strength of columns was demonstrated. The result of these researches led to a revision of the formulæ at present in use for the design of columns. It was shown that some provision requires to be made for the wave deformation due to secondary flexure, which depends upon the thickness and the figure or shape of the cross-section of the column. A modified formula based upon the experimental work and theory was then given which took into consideration this variable. The object in view in the present paper was the determination of the constants to be used in the formulæ for the design of solid round-ended columns. A series of experiments were carried

* References to previous papers:—"The Strength of Columns," Proceedings of the Institution of Mechanical Engineers, June, 1905; "The Economic Design of Columns," Proceedings of the Institution of Civil Engineers of Ireland, March, 1907; "The Design of Struts," *Engineering*, January 10, 1908.

out on columns of cast-tool steel, Bessemer steel, mild steel shafting rolled and annealed, wrought iron, and cast iron. The results obtained are recorded on three diagrams, on which are plotted five curves obtained from the experiments, together with the stress-strain diagram from the different materials and a series of curves plotted to the formulæ adopted.

Some interesting results have been obtained as to the effect of the ductility of the material, its strength at the yield point, and Young's modulus of elasticity upon the strength of solid columns. A comparison is then given of the Rankine-Gordon and other formulæ. The constants to be used and as adopted from the experiments are then discussed.

Recent Advances in Steam Turbines.—*By* GERALD STONEY, *B.E., M.I.C.E., M.I.E.E.* Two years ago a paper was read by the author at the York meeting of the British Association, and this paper was a continuation of the former, and showed the rapid progress that has been made in steam turbines during the past two years. Improvements in electric machinery, both in the form of continuous-current dynamos and also alternators driven by steam turbines, were dealt with, as well as the application of the steam turbine to air compressors and for pumping water, &c. The use of exhaust-steam turbines for utilising the exhaust from reciprocating engines was described, and also of intermittent supplies of exhaust steam, such as from winding-engines, by help of thermal accumulators, along with the further refinement of mixed-pressure turbines, where, when the supply of exhaust steam fails, a high-pressure portion is brought into action, thus utilising the full boiler pressure. Improvements in condensers and the various means to obtain the highest vacuum possible were also described.

In marine work the great development of express liners on the Atlantic was dealt with, and also the combination of reciprocating engines and turbines for tramp steamers and vessels of low speed, from which combination greater economy can be obtained than with either steam turbines or reciprocating engines alone.

Report of the Committee* on Gaseous Explosions.

GENERAL SCOPE OF THE REPORT.

To engineers the investigation of gaseous explosions is chiefly of interest because of its bearing upon the theory of the internal combustion engine. The Committee have hitherto considered it mainly from this point of view, conceiving that a limited interpretation of their reference would be necessary if their labours were to lead to any result within a reasonable time, and that the limitation adopted should be determined by the fact that the Commtttee was initiated by the Engineering Section. On the other hand, the work has been by no means entirely, or even mainly, of a practical, as distinct from a purely scientific, character. The test of practical value or interest has only been applied for the purpose of selecting from among the large number of questions arising in connection with explosions those which are proper subjects for investigation by this Committee, not with the idea of limiting such investigation to the practical aspect of these questions.

The essential feature common to the operation of all gas engines is the conversion of a mixture of inflammable gases, by combustion or explosion, into a mass which consists in all practical cases of a mixture of steam, carbon dioxide, nitrogen, and excess oxygen. The performance of the engine depends primarily on the change in pressure or volume, or both, resulting from this chemical transformation, and on the properties of the products of the transformation after they are formed. It depends in only a secondary degree on the nature of the chemical process and on the velocity with which it takes place. These matters, important though they must be in any investigation of explosions and in the theory of the gas engine, are not of the first importance. The foundation must be a knowledge of the properties of the gases enumerated above at the temperatures occurring in the gas engine—that is, between 1,000° and 2,500° C. This report, therefore, consists mainly of an analysis of the present state of knowledge on this subject, together with suggestions as to the directions in which further research may be undertaken with the object of advancing it. It will be found, however, that as the mechanism and the velocity of combustion must be taken into account as disturbing factors when applying our knowledge of the gases in the theory of the gas engine, so they enter into many of the experiments on which that knowledge is based, and some discussion of them is indispensable in any criticism of these experiments.

Thermodynamic theory shows that the physical properties of a gas in chemical equilibrium are completely specified when—

(1) The relation between the pressure and volume at constant temperature is known, and

* Consisting of Sir W. H. Preece (Chairman), Mr. Dugald Clerk and Professor Bertram Hopkinson (Joint Secretaries), Professors Bone, Burstall, Callendar, Coker, Dalby, Dixon, Hele-Shaw, Smithells and Watson, Dr. Harker, Lieut.-Colonel Holden, Dr. Petavel, and Captain Sankey.

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(2) The internal energy per unit volume is given as a function of the temperature and the density.

The energy of a gas per unit of mass at temperature θ is usually defined as $k\ (\theta - \theta_0)$, where θ_0 is the standard temperature from which energies are reckoned, and k the mean specific heat at constant volume between the temperatures θ_0 and θ. The second of these data is therefore equivalent to a knowledge of the specific heat in the terms of the temperature and density. This form of statement is probably more familiar; but, for reasons given later on, it is in many ways less convenient than that based upon the energy function.

For those gases with which we have to deal it may be assumed that the first relation is that given by Boyle's law. Experiment and theory alike point to the conclusion that deviation from this law only occurs when the density of the gas departs widely from its normal value, and that it is diminished by high temperature. In the gas engine the density of the gas rarely exceeds ten times that of the atmosphere, a point at which the deviation from Boyle's law in air (at 100° C.) is only about one-half per cent.*

It is usual to make the further assumption that the product pv is proportional to the absolute temperature θ. A detailed examination of the grounds of this assumption forms the subject of a section of this report. At this point it is only necessary to notice that, if it be true, then the internal energy is a function of the temperature only, and is independent of the density. If, on the other hand, the perfect gas law does not hold, the true relation between pv and θ can be deduced from a knowledge of internal energy, which is in that case a function both of the temperature and of the density.

The properties of the gases with which we have to deal are therefore completely defined when the energy has been tabulated as a function of the temperature and the density. So far as the present state of knowledge goes the energy is to be expressed in terms of temperature only; but an important part of future investigation must deal with its dependence on the density either by direct measurement or by a determination of the relation between pv and θ at high temperatures.

The prediction of the temperature reached in combustion, which must be the starting point of any investigation of explosions, also rests primarily upon a knowledge of the energy function. For, subject to corrections for loss of heat, incomplete combustion, and work done while combustion proceeds, the thermal energy of the mixture of steam, CO_2, &c., after combustion is equal to the chemical energy of the gases from which that mixture was formed. The latter can be accurately inferred from the composition of the combustible gases, and, the thermal energy being thus known, the temperature can be calculated from a table of the energy function. The pressure or volume changes resulting from combustion can be deduced from the temperature by the use of the $p - v - \theta$ relations, which again ultimately depend upon the form of the energy function. A table of this function at high temperatures is therefore the first datum necessary for the investigations entrusted to the Committee, and is the principal subject of this report. Before proceeding to the discussion of this physical question, however, it is well to say something further about its bearing on practical engineering problems.

The first requisite for predicting the performance of a gas engine is to know the rise of temperature and the consequent rise of pressure produced by the explosion. The importance of this need not be insisted upon; it is not only the principal factor in the mean pressure developed, it also determines in large measure the mechanical design of the engine and the necessary strength of its parts. The part played by the energy function in the calculation of this rise of pressure has been indicated in the last paragraph. In proceeding further to analyse the indicator diagram given by the engine with the object of accounting at each point for the heat which has been put in, a knowledge of this function is again required. The heat accounted for on the diagram is the work which has been done *plus* the heat contained in the gas. The latter item can be calculated from the temperature if the energy function be known. The balance unaccounted for, which it is usually the object of such investigations to find — whether in the steam engine or the gas engine—is the heat which has been lost to the walls or has been suppressed owing to incomplete combustion. In fact, the internal energy of the gases at high temperatures plays much the same part in the analysis of gas engine phenomena as does the total heat of steam in investigating the working of the steam engine.

Again, from a table of internal energy it is possible to predict the pressure changes resulting from any series of operations such as occur in the gas engine, one item in which is an explosion subject to certain hypothetical conditions which cannot be realised in practice, though they can be indefinitely approached. An ideal diagram of this kind, corresponding to the cycle of operations which is most usual in present-day gas engines, can, for example, be constructed for any given combustible mixture on the assumption that the combustion is instantaneous and complete at the in-centre: that there is no loss of heat in compression, explosion, or expansion; and that during expansion the gases are at all times in thermal and chemical equilibrium. These conditions can never be completely realised, but can in theory be approached asymptotically by improvements in design carried on within certain defined limits — namely, that the degree of compression and the nature of the mixture are to be unaltered. For example, the heat loss may be reduced by increasing the size

* See Witkowski, *Phil. Mag.*, vol. xli. (1896), p. 309.

of the engine and altering the nature of the cylinder walls, and the attainment of thermal and chemical equilibrium may be promoted by reducing the speed. Such an ideal cycle is, in fact, precisely analogous to the Rankine cycle of the steam engine, in that it takes account of the actual physical properties of the working substance but leaves out of account such non-essential imperfections as heat loss to the cylinder walls. It represents an ideal which the real engine may approach indefinitely but can never attain; and the closeness of the approach is a true measure of the perfection of the engine.

The ideal cycle which has hitherto been used in discussing the performances of gas engines is the well-known air cycle. This is based upon a special assumption as to the form of the energy function—namely, that it is a linear function of the temperature at high, as it is known to be at low, temperatures. The specific heat of the working substance is taken to be constant and equal to 19 ft. lbs. per cu. ft. or 4·8 calories per gramme molecule. In the state of ignorance as to the real form of the energy function which prevailed until quite recently, this assumption was as good as any other, since it was impossible to say that the value of the energy derived from it was further from the truth than any other value which might be assigned to it. So far as was known, the differences between the indicator diagram of a real engine and the corresponding air-cycle diagram might have been wholly, or almost wholly, due to what have been called above "non-essential imperfections"—that is, to heat loss and to incomplete combustion. In other words, there was no conclusive evidence that the air cycle was not for practical purposes a true ideal cycle in the sense defined above and equivalent to the Rankine cycle for the steam engine. Under these circumstances its extreme simplicity made it the best available standard of comparison for judging the performance of a real engine. Recent researches, however, on the properties of the gases at high temperatures have definitely shown that the assumption of constant specific heat is erroneous, and have given sufficient information about the magnitude of the error to show that it is of material importance. They have shown that the air cycle cannot be regarded as equivalent to the Rankine cycle in the steam engine, inasmuch as it does not take account of the properties of the actual working fluid, but postulates a hypothetical fluid which has no real existence. It is as though in the theory of the steam engine the total heat of the steam were to be taken as equal to its latent heat, the sensible heat of the water being neglected. This assumption would lead to a simpler formula for the ideal efficiency for the steam engine, but it would be erroneous in the same way and to about the same extent as the air cycle formula for the gas engine.*

* If the sensible heat of the steam can be neglected in comparison with its latent heat the Rankine cycle reduced to the Carnot cycle, with efficiency $\frac{T_1 - T_2}{T_1}$, for no heat is necessary to warm the water from the condenser to the boiler temperature, and the whole process becomes reversible. The efficiency of the Carnot cycle usually exceeds that of the corresponding Rankine cycle by about one eighth part.

The closer approximation to the real cycle which is made by taking account of the actual properties of the working fluid—in the steam engine the total heat of the steam instead of only the latent heat, in the gas engine the true value of the energy instead of that based on the assumption of constant specific heat—though it leads to some complication of formulæ, gives compensating advantages of real practical value. It shows the engineer what are the limits to the improvements which can be effected by changes of design or increase of size, and it enables him to judge whether it is better that the lines of development should proceed in such directions or in the direction of radically modifying the cycle of operations.

Measurement of the Internal Energy or Specific Heats of Gases at High Temperatures.

The results of most experiments on the energy of gases have been expressed in the form of tables or formulæ giving the specific heat (referred to in unit mass of the gas) in terms of the temperature. It would appear preferable for most purposes to exhibit them in terms of internal energy per unit volume. That is the form most convenient for purposes of thermodynamic calculation, and it has the further advantage that it expresses the actual quantity measured. In nearly all the experiments on the specific heat of gases the increase of energy in unit volume associated with a large rise of temperature is measured; and in most the lower limit of temperature is near that of the room. The rate of change with temperature, of the energy so determined, is sometimes called the "true" or "instantaneous" specific heat, and sometimes "thermal capacity." The committee are of opinion that a definite name should be given to this important quantity, and they suggest the name "volumetric heat," which if adopted should include in its significance that the measurement to which it relates is made at constant volume, and is referred to unit volumes of the gas. The term "specific heat" could then be restricted to its usual meaning, which refers to unit mass of the substance. Convenience of calculation is promoted if the unit of volume taken is that corresponding to the gramme molecule under standard conditions, which is sufficiently nearly the same for each of the gases under consideration and equal to 22·25 litres.* In this report

* The volumes of the gramme molecule for the several gases are:—

H_2	22·24	CO	22·21
N_2	22·28	CO_2	22·08
O_2	22·22		

in litres at 0° C. and under a pressure of 760 mm. of mercury.

It may be mentioned here that 1 calorie per gramme molecule is equivalent to 3·96 ft. lbs. per cu. ft.

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internal energy and volumetric heat are expressed as calories* per 22·25 standard litres; and the zero of temperature from which the energy is reckoned (except where otherwise stated) is taken to be 100° C., in order that steam may be included on the same basis as the other gases. The results are conveniently exhibited as curves, in which the energy is the ordinate, and the excess of the temperature over 100° C. is the abscissa. The slope of such a curve represents the volumetric heat C., and the ordinate divided by the abscissa for any temperature represents the mean volumetric heat from 100° C. to that temperature, here denoted by C.

The experimental work done on this subject may be divided into three classes:—

(1) Constant-pressure experiments: Regnault, Wiedemann, Witkowski, Lussana, Holborn and Austin, Holborn and Henning. The gas is heated from an external source in these experiments, and is at atmospheric pressure.

(2) Experiments in which both volume and pressure are varied, the gas being heated by compression. The recent experiments of Clerk and the determinations of the velocity of sound in hot gas by Dixon and others belong to this class.

(3) Constant-volume experiment. To this category belong the explosion experiments of Mallard and Le Chatelier, Clerk, Langen, Petavel, Hopkinson, and others, and Joly's determinations with the steam calorimeter. In the explosion experiments the gas is heated by internal combustion.

(1) Constant-pressure Experiments.

The constant-pressure experiments have been carried to a temperature of about 1,400° C. The gas under atmospheric pressure flows steadily through a heater and then through a calorimeter, where it is cooled. The temperature just before entering and just after leaving the calorimeter and the quantity of heat evolved per gramme molecule of the gas are measured. This quantity of heat less the work done in the contraction, which is 1·98 times the fall of temperature, is the change of internal energy corresponding to that fall.

Regnault applied the method to air, H_2, CO, CO_2, and other gases over the range 0—200° C.

Wiedemann† repeated Regnault's experiments with some modifications of the apparatus. On account of the small range of temperature these experiments must be regarded as only giving the slope of the internal energy curve at the origin; but as they give this with an accuracy at least equal to that with which the ordinate is known at higher temperatures, they are of considerable importance in constructing the curve. The following table shows the values of the mean volumetric heat C. over the range 0° to 100° C., found by these two observers for air, H_2 and CO. Witkowski's value for air, by the same method, is in exact agreement with Regnault's.*

	H_2.	CO.	Air.
Wiedmann... ...	4·84	4·81	4·90
Regnault	4·84	4·88	4·86

These results give a good idea of the accuracy attained in these experiments. In both sets the different observations ranged about 1½ per cent. above and below the mean in each determination. Later work shows that the value of C air is probably about 1 per cent. greater over the range 0 to 209 than over the range 0 to 100. Regnault was unable to detect this difference, though he looked for it.

The volumetric heat of air has also been determined by Joly by means of the steam calorimeter. He found the specific heat of air at constant volume for the range 10° to 100° C., and at a pressure of about 20 atmospheres to be 0·172. There were distinct signs of an increase of specific heat with density, and assuming this to follow the linear law given by Joly, the specific heat at normal density would be 0·1715, equivalent to 4·93 calories per gramme molecule. Professor Callendar points out, however, that this is based upon Regnault's number for the latent heat of steam, which is of doubtful accuracy, and that more probably Joly's determination when reduced to the 15° calorie should be 0·1732, or 4·98 calories per gramme molecule. According to some unpublished experiments by a constant-pressure method which have been made by Mr. Swann in Professor Callendar's laboratory, and in which it is believed that some sources of systematic error inherent in the earlier experiments of this type have been avoided, the volumetric heat of air is 5·0. These results are distinctly higher than those obtained by Wiedemann and Regnault, but the difference is of no importance for the present purpose except as an indication of the possibility of systematic errors in their method of experiment, which may become important when it is applied to higher temperatures. It may be taken as fairly certain that the volumetric heat of air at 100° C. is within 2 per cent. of 4·9.

In the case of CO_2 the results obtained by Wiedemann and Regnault were:—

		W.	R.
Increase of internal energy	0 to 100	710	680
,, ,, ,,	0 to 200	1,510	1,490
,, ,, ,,	100 to 200	800	810

The first two rows of figures represent practically the quantities actually measured in these experiments. The third is obtained by difference from the first two, and is therefore

* There is some difference in the energy value of the calorie according to the temperature at which it is measured. The difference between the maximum and minimum value over the range 0° to 100° C. amounts to about 1 per cent. This is of no importance for the purposes of this report, except in one or two places; but where it is necessary to be so precise the calorie at 15° C.—namely, the quantity of heat required to warm 1 gramme of water from 14½° C. to 15½° C.—is meant.

† *Annalen der Physik*, 1876, vol. clvii.

* *Phil Mag.*, vol. xlii. (1896).

affected with a greater probable error than either. The result of the two sets of experiments may be summed up by saying that the volumetric heat of CO_2 at 100° C., taken as an equal to the mean volumetric heat between 0° and 200° C., is between 7·45 and 7·55, and that its rate of increase with temperature is between 0·009 and 0·013, or roughly, $\frac{1}{100}$th part per deg. C. The specific heat of steam at constant (atmospheric) pressure in the neighbourhood of 100° C. according to Regnault's, is 0·48, equivalent to 6·64 volumetric heat, and subsequent observers have shown that this value is at least as accurate as Regnault's value of the specific heat of air.

From Joly's experiments with the steam calorimeter, when corrected according to Callendar for the error in Regnault's value of the latent heat of steam, the specific heat of CO_2 between 10° and 100° C., and at a pressure of 12 atmospheres, is 0·172, and it increases by 0·25 per cent. per atmosphere. Assuming this law of increase to hold between one atmosphere and 12 atmospheres, the mean specific heat at normal density for the range 10° to 100° should be 0·1666, and the volumetric heat should be 7·3, which is again decidedly greater than the values obtained by Wiedemann and Regnault. According to a recent determination by Swann, the result of which has been communicated to the Committee by Professor Callendar, the volumetric heat of CO_2 at 100° is 7·76—again materially higher than Regnault and Wiedemann (7·5).

Holborn in conjunction with Austin carried the constant-pressure determinations for air and CO_2 up to 800° C. The gas was heated electrically and the temperature was measured with a thermo-couple. Similar measurements on steam were made by Holborn and Henning, who subsequently carried the determinations for the three gases up to 1,400° C[2].

Holborn and Henning express the results of all these experiments in algebraical formula representing the mean specific heats of CO_2, air, and steam respectively over the range $0-\theta$ in the case of the first two gases, and $100-\theta$ in the case of steam. From these formulæ the full-lined curves in Fig. 1, exhibiting the internal energy, have been constructed. The actual observations are also shown in the same figure. Each of these observations represents the mean of a large number of experiments, in some cases as many as thirty. The results of the individual experiments in such a group ranged about 2 or 3 per cent. above and below the mean. These casual errors would no doubt cancel out to a great extent in taking the mean, which, apart from systematic errors inherent in the method of experiment, is probably correct within about 2 per cent.

This degree of accuracy is not sufficient to enable any deduction to be made as to the manner of variation of the volumetric heat beyond a rough estimate of its average rate of increase over the whole range of experiment. The following are the values of the volumetric heats of air, steam, and CO_2, at 100°, 600°, and 1100°, respectively:—

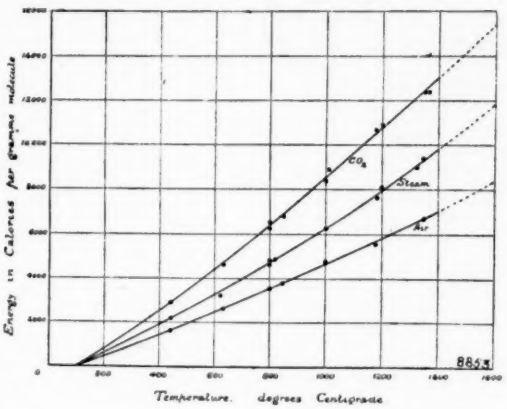

FIG. 1.

—	100°		600°		1,100°		Increase 100—1,100.
	C.	γ.	C.	γ.	C.	γ.	
Air ...	4·9	1·404	5·2	1·38	5·75	1·345	0·9
Steam	6·6	1·30	6·85	1·29	8·5	1·24	1·9
CO_2 ...	7·5	1·26	9·95	1·20	11·1	1·18	3·6

The corresponding values of γ are also shown:

$$\gamma = 1 \frac{1{\cdot}98}{C}.$$

The values at 100° are derived from the experiments of Wiedemann and Regnault. Those at 600° and 1,100° are based on the specific heat values given by Holborn and Henning; in other words, they are obtained by drawing tangents to the curves, Fig. 1. The error at these higher temperatures may be double that of the internal energy, or, say, 4 per cent. The figures show that the volumetric heat of air increases by about ·0009, that of steam by ·0033, and that of CO_2 by ·0036 per degree Centigrade over the range 100°—1,100° C. There is no evidence that the rate of increase is other than constant in the case of air; but there can be no doubt that the average rate of increase between 100° and 1,100° in CO_2 is less than half the rate of increase between 0° and 200°, as determined by Wiedemann and Regnault. There is also distinct evidence in these and other experiments that the rate of increase of the specific heat of steam becomes greater as the temperature rises.

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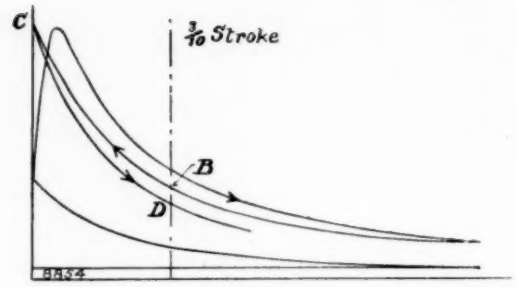

FIG. 2.

Clerk's Experiments.

These cover about the same range of temperature as Holborn and Henning. The gas used was the products of an explosion in a gas-engine, and therefore consisted of a mixture of CO_2 steam and air. It was first expanded in the ordinary course after the explosion, and was then heated by compression on the next in-stroke of the engine, the valves being kept closed for this purpose. On the next out-stroke the gas was again expanded, then compressed again, and so on, the valves remaining closed and the engine running on its own momentum. An indicator diagram was taken of the whole operation. The change of internal energy in any portion of the compression stroke (*e.g.*, *B C* in Fig. 2) is equal to the work done less the heat lost to the cylinder walls; in an expansion stroke (*C D*) it is the work done *plus* the heat lost. The work can be obtained from the indicator diagram with an accuracy which is only limited by the indicating appliances. The change of temperature can also be calculated from the indicator diagram, subject to a knowledge of the temperature at one point. Errors in the latter, however, do not greatly affect the results found for internal energy or volumetric heat, because the figure for the quantity of gas present is affected by these errors in such a way as to cancel out the error in temperature interval.

The loss of heat comes in as a correction on the work done and was estimated by a comparison of the compression line and the immediately following expansion line (*B C* and *C D*, Fig. 2). The calculation is based on the assumption that the total heat loss from the hot gases during any given portion of a stroke is the same in expansion and compression if the mean temperature be the same.

In the first compression the temperature of the gas rose to about 1,100° C. (at the point *C*, Fig. 2). During the first three-tenths of the following expansion stroke (*C D*), the temperature fell to about 700° C. The work done in this part of the expansion was measured and the heat loss determined as above was added. Thus the change of internal energy corresponding to the temperature change 1,100°—700° is obtained. The average volumetric heat over this range is within the errors of experiment equal to the volumetric heat at the mean temperature of 900° C., which accordingly is by this method determined direct instead of by difference, as is necessarily the case when (as in Holborn and Henning's experiments) the whole internal energy change associated with complete cooling of the gas is measured.

In view of the great difference in the method of experiment a comparison of Clerk's results with those of Messrs. Holborn and Henning is of great interest. Clerk's measurements extended to 1,450° C., but those above 1,200° C. were based on the first expansion line after the explosion when the method for getting heat loss would be of doubtful application, and when, moreover, combustion may have been incomplete. It will be better therefore to confine the comparison to temperatures of 1,200° and below. The following table exhibits the internal energies of the mixed gas with which Clerk experimented calculated from Holborn and Henning's figures, together with the energy calculated from Clerk's values for the mean volumetric heat. The energies are, as usual, reckoned from 100° C.; and the energies of an ideal gas with a constant volumetric heat of 4·9 are added for comparison.

Temperature.	Holborn and Henning.	Clerk.	Ideal Gas.
400	1,580	1,720	1,470
800	3,840	4,300	3,430
1,200	6,285	7,040	5,390

It will be seen that Clerk's results are throughout about 10 per cent. higher than the others. The difference between the energy of the real and of the ideal gas, the discovery of which is the true object of these experiments, is about twice as great in the one case as in the other. It does not seem possible to account for so large a discrepancy by ordinary experimental errors. It must be due either to some systematic error inherent in the method of experiment in one or both cases, or to a difference in the conditions of experiment giving rise to a real difference of internal energy.

Professor Callendar has favoured the commitee with a note dealing with the constant-pressure experiments. He is of opinion that the results obtained by Regnault's method are too low, and that at the higher temperatures reached by Holborn and Henning the error may possibly amount to as much as 10 per cent. In all these experiments there is a considerable

flow of heat from the heater to the calorimeter. This, of course, has to be deducted from the heat registered in the calorimeter in order to find that which has been given up by the hot gas. All the experimenters by this method have determined the amount of this correction by observations when the gas was not flowing, or have compensated it under the same conditions by radiation from the calorimeter, making, in either case, the assumption that the amount of heat conducted is the same whether the gas be flowing or not. Prof. Callendar is of opinion that this heat-flow is, in fact, much less when the gas is flowing. He considers that even the values obtained by Regnault may be as much as 2 or 3 per cent. too low, and he supports this contention by reference to the work of other experimenters (some of which has been alluded to above) and by theoretical considerations. As this type of error is likely to increase greatly with rise of temperature a systematic error of even 2 per cent. in Regnault's results, if established, would give reason to suspect that the experiments at high temperatures may be subject to errors of real importance for the present purpose.

If there be systematic error in Mr. Clerk's work it seems most likely that it lies in the estimate of heat loss. The total heat loss in the first partial compression and expansion line in the diagram (*B C D*, Fig. 2) is estimated from the fall of temperature and from the net work done (area *B C D*) in the double operation, and amounts to, roughly, half the work done in expansion. This loss has to be divided between compression and expansion, and Mr. Clerk divides it on the assumption that if the mean temperature in compression and expansion were the same the heat loss would also be the same. The mean temperature in expansion is, in fact, rather less than in compression, and the heat loss calculated in this way is correspondingly smaller, but the difference on this account is not very great, and the result is, roughly speaking, that the loss is equally divided between the two operations. Thus the correction to be added to the work done in expansion in order to get the total loss of energy of the gas is about 25 per cent. of the work, or 20 per cent. of the energy change.

Prof. Hopkinson has dealt with this point in a note which he communicated to the Committee, and he is of opinion that, relative to the mean temperature, the heat loss is really much greater in compression than it is in expansion. He supports this view by reference to some experiments which he has made on the compression and expansion of a charge of cold air in a gas engine which was motored round with the gas cut off. The specific heat of air being known, the loss of heat in any part of the compression or expansion stroke can in this case be independently estimated from the diagram. He found that while in the latter half of the compression stroke the heat lost to the walls amounted to a considerable fraction of the work done, some part of this loss was actually restored to the gas during the first half of the succeeding expansion, and this notwithstanding the high temperature of the air, which in expansion, as in compression, was much above that of the walls. An estimate of the thermal capacity could, of course, be obtained from this diagram by the application of Mr. Clerk's method, and it would lead to a result considerably in excess of the truth. Mr. Clerk has himself tried this same experiment of compressing and expanding air, and he also has found that the resulting value of the specific heat of air is too high and that the air takes in heat during expansion. Prof. Hopkinson thinks it possible that the heat lost during the partial compression line in Clerk's diagram may be more than twice as great as the loss during expansion. If this were so, the correction for heat loss in expansion would be less than 16 per cent. of the work done instead of 25 per cent., and the true change of energy would be less than that calculated on the assumption of equal heat-loss in compression and expansion by 7 per cent. or more.

It will be seen that the errors believed to affect each method of experiment are in such a direction as to account for the divergence of the results; and it is quite probable that when these errors are completely allowed for, the discrepancy will largely disappear. Meanwhile the internal energy of the products of combustion in the gas engine at 1,200° C., if taken as the mean of Clerk's and of Holborn and Henning's results, must be regarded as subject to a possible error of about 5 per cent.

Under these circumstances it does not seem necessary to discuss the possibility that there may be a real difference between the energy values obtained by the two methods due to the different conditions of experiment. It may be pointed out, however, that Clerk's gas was at the maximum temperature from fifteen to twenty times as dense as Holborn and Henning's. The difference in the condition of the gas is such that a comparison of the results obtained by the two methods, when freed from experimental errors, will be of great interest and importance.

Explosion Experiments.

If a combustible mixture of gases be fired in a closed vessel impervious to heat, and if sufficient time elapse to allow of the attainment of a complete thermal and chemical equilibrium, the internal energy of the products of combustion after the explosion will be equal to the chemical energy before explosion. The latter is capable of accurate measurement. The temperature reached after explosion can be inferred from the pressure, assuming the gaseous laws to hold. The pressure can also be measured without difficulty and with considerable accuracy.

In the study of explosion pressures we have therefore a very convenient and simple means

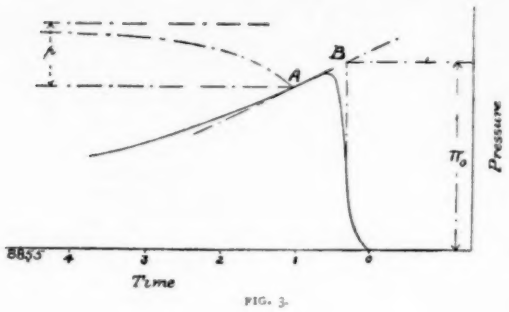

FIG. 3.

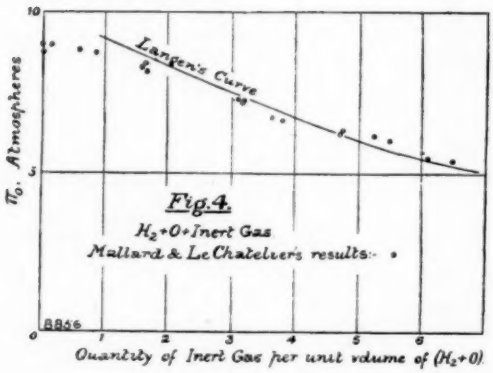

Fig. 4.

H_2+O+ Inert Gas

Mallard & Le Chatelier's results. •

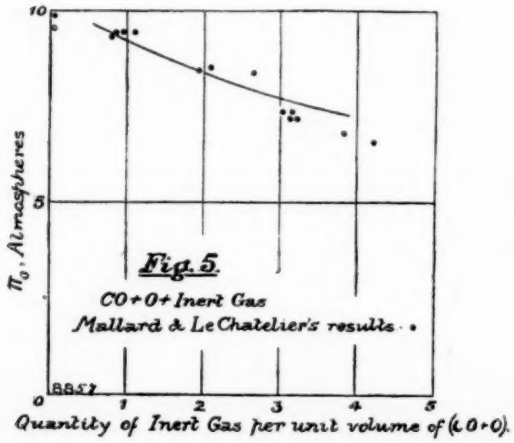

Fig. 5.

$CO+O+$ Inert Gas

Mallard & Le Chatelier's results. •

of getting the internal energy function at high temperatures, provided that it is possible to make the necessary corrections for deducing from the pressures observed in a real explosion the pressure reached in an explosion under ideal circumstances postulated above. Moreover, the gaseous laws on which the temperature estimations are based can themselves be checked, and if necessary corrected, by comparison of the pressures reached by mixtures of the same compositions, but of different densities. Thus explosion experiments are capable of furnishing a complete account of all thermal properties of gases at the temperatures reached by combustion, subject always to the possibility of making the corrections referred to above. The difficulty of finding these corrections is, however, very great, and, in consequence of the uncertainty which prevails even as to their order of magnitude, the large amount of work which has been done on explosion pressures gives but little definite information as to the specific heat of gases. Nevertheless it is to the study of explosion pressures that we owe such knowledge as we possess of the energy function at the temperatures which prevail in the gas engine, and it is to work on these lines that we must look in large measure for extension of our knowledge. A full discussion of what has already been done must therefore form an important part of this report.

Let H be the calorific value of the mixture before combustion, let h be the heat lost at some point A on the record (taken on a revolving drum) connecting the pressure and the time (Fig. 3). The energy in the gas is the $H-h$. The gas at this point is, however, certainly not in thermal equilibrium, and is probably neither in chemical equilibrium nor at rest. If, therefore, the loss of heat were suddenly arrested at A, the pressure would change owing to the more or less gradual attainment of equilibrium in all three respects. The equilibrium value of the pressure would be reached asymptotically, as shown by the dotted lines. When equilibrium has been attained, the energy of the gas is all thermal and equal to $H-h$, and the temperature can be calculated in the ordinary way from the pressure. The problem, therefore, is first to find or estimate the heat-loss, h, which has occurred at some point on the explosion record, and then to find or

estimate by how much the equilibrium value of the pressure, if there were no further heat-loss, would differ from that shown on the record.

This change of pressure, marked p on the diagram, is due partly to the combustion of the gas remaining unburnt at A, and partly to the equalisation of temperature by convection. It may also be due to some extent to the damping down of the motion of the gas set up by the explosion.

The sooner the point A is taken, the less will be the loss of heat: but the greater, on the other hand, will be the departure from equilibrium conditions. The principal workers in this field, Mallard and Le Chatelier and Langen, assumed that the latter might be neglected if the point A were taken at the point of inflexion on the falling curve, and they estimated the loss of heat by prolonging this curve backwards, as shown. They assumed, in fact, that the pressure given by the point B was that which would ultimately have been reached had the explosion taken place in a vessel with walls impervious to heat.

It is difficult to justify this procedure on *à priori* grounds; the only satisfactory justification is to show, by independent evidence, that it leads to correct results. The main object of this section of the report is to examine such evidence as there is of this kind, to point out the defects in it, and to suggest experimental methods by which they could perhaps be remedied.

In so far as the heat-loss and the departure from equilibrium are dependent on surface phenomena, a definite estimate of their amount can be obtained by a comparison of explosions of the same mixture in vessels of different sizes.

Many years ago Berthelot tried this experiment, firing hydrogen and oxygen, in explosive proportions, in vessels of 300 c.c. and 4000 c.c. respectively. It is stated that the pressure reached was very nearly the same, which would show that such part of the cooling and other corrections as depends on the surface of the vessel is small in the case of this mixture.

Materials for a more accurate comparison are to be found in the extensive researches of Mallard and Le Chatelier, and of Langen. The French experimenters worked with a cylindrical vessel 17 cm. × 17 cm., whereas Langen used a sphere 40 cm. diameter. The ratio $\frac{\text{surface}}{\text{volume}}$ was 2·3 times as great in the first as in the second case.

The table below shows the results obtained in two instances, in each of which the composition of the mixture was practically identical in the two sets of experiments:—

π_0 is the pressure reached in the explosion in atmospheres after correcting for cooling in the manner described above, when the initial temperature is 0° C. The cooling correction, or excess of the pressure at B (Fig. 3) over that at A, is shown in the last column. Figs. 4 and 5, which are taken from Langen's paper, show a comparison between the curve adopted by Langen, as representing the results of his experiments, and Mallard and Le Chatelier's observations.

Mixture.	Observer.	π_0.	Cooling correction.
			per cent.
2 vols. air ...	Mallard and Le Chatelier ...	7·40	14
1 vol. (H_2+O)	Langen... ...	7·50	8
2 vols. air ..	Mallard and Le Chatelier ...	7·50	7½
1 vol. (CO+O)	Langen... ...	7·50	8

On the whole the agreement between the two sets of experiments is very fair, and the deviations are not such as to suggest that any very great error has been made in estimating such part of the corrections for heat-loss, or for unburnt gas, as depend on the surface of the vessel. If, for example, Langen were, on the average, 4 per cent. out from this cause, Mallard and Le Chatelier would be 9 per cent. out, and would differ by 5 per cent. from Langen. Differences of that amount do occur, but they do not seem to be systematic. Further experiment of the same kind on vessels with a greater difference of size but of similar geometrical form is, however, desirable.

The question remains, how far the corrections really are surface corrections. This appears to the Committee to be the most important question of a general character awaiting solution in connection with gaseous explosions when regarded as a means of investigating the properties of gases at higher temperatures. It will be convenient to discuss each of the corrections enumerated above with special regard to this question.

Loss of Heat.—That much of the heat-loss goes on by direct conduction to the walls, and is, therefore, a surface phenomenon, is obvious. But there is reason to believe that the loss by radiation, which certainly exists in any flame, is practically important.

(*a*) Measurements of the temperature reached in an explosion by means of a platinum thermometer, under circumstances which render very improbable any loss of heat by conduction from the gas whose temperature is measured, show that that temperature is considerably lower than is to be expected from the heat of combustion of the gases and the specific heat of the products. Professor Callendar pointed out, in the discussion of these experiments, that there was probably a good deal of radiation, and stated that he had found that an ordinary Bunsen flame might radiate up to 15 per cent. of its heat.*

(*b*) Recent experiments, in which the loss of heat during an explosion was directly measured by finding the rise of temperature of the walls, showed that in a certain coal-gas explosion it amounted to about 12 per cent. of the whole heat at the moment of maximum pressure.

* *Proc. Roy. Soc.*, vol. lxxvii., p 400.

Estimated by Mallard and Le Chatelier's extrapolation method, the loss was at most 5 per cent.*

The prevailing opinion seems to be that most simple gases cannot be made to radiate by direct heating. If this be so the radiation must take place in the act of combustion. It seems very probable that when, say, hydrogen and oxygen combine a certain part of the energy of combination passes into the form of internal vibrations of the steam molecule, and that a large proportion, if not all, of this part is ultimately radiated away. If this be the case a definite proportion of the heat produced in combustion is always lost, and a comparison of explosions in vessels of different sizes would not reveal this loss.

(*To be concluded.*)

* *Proc. Roy. Soc.*, A, vol. lxxix. p. 147.

LEADING ARTICLES OF THE MONTH—CURRENT EVENTS—TECHNICAL INFORMATION FROM ALL SOURCES.

The Hydro-Electric Plant of the Canadian Niagara Power Company * — The plant, as now constructed, consists of a canal, wheel-pit, and tunnel having a designed capacity of 100,000 e.h.p., an installation of five units, each having a nominal capacity of 10,000 e.h.p., and a power-house sufficiently large to enclose them.

In order to obtain a sufficient depth of water at the entrance to the canal, a portion of the river bottom had to be unwatered, that the necessary excavation might proceed. It was a perilous matter to obtain in the rapids the soundings which were requisite for a determination of the limits within which such excavation was necessary, but, after several narrow escapes in the turbulent current leading to the Horseshoe Falls, the necessary soundings were obtained and a coffer-dam constructed to enclose the desired area, a depth of 15 ft. of water being secured where possible.

The water areas are selected so that, when 100,000 h.p. is being generated, the average velocity, with the water at low stage, will be 2·7 ft. per sec. at the entrance, and 2·35 ft. per sec. through the submerged arches separating the forebay from the main canal. The position of the plant below the principal cascades of the river makes the ice conditions difficult, as the Niagara River carries large quantities of ice during the greater part of the winter.

The plant is protected against ice: (*a*) By a rack of 2-in. inclined bars, spaced 12 in. apart, connected with a steel framework carried by the masonry piers at the entrance to the canal. This rack extends 4½ ft. below the normal water surface. For the purpose of keeping out fine ice, steel plates were afterwards attached to the rack. (*b*) The outer wall of the forebay superstructure rests on a wall of first-class masonry containing a series of arched openings 19 ft. 9 in. wide, the crown of each arch being 2·1 ft. below low water. This arrangement of submerged openings effectually prevents the entrance into the forebay of floating ice and such other floating material as may find its way into the canal. (*c*) To provide means for removing from the canal such finely-divided ice as might pass the outer rack, a discharge channel was built. A difference of level of about 2 ft. is obtained between the surface of the water in the sluiceway and that in the canal. The sluiceway has a width of 16 ft. and a depth of water of about 10 ft. It is separated from the canal by masonry weirs, the tops of which are 2·2 ft. below low water. A further separation is effected by steel gates sliding in cast iron grooves. Two openings, each 16 ft. in width, provide for the flow of ice from the canal, while a third gate of similar construction allows the escape of any material which may enter the

* Abstracted from a paper read before the American Society of Civil Engineers, October, 1908.

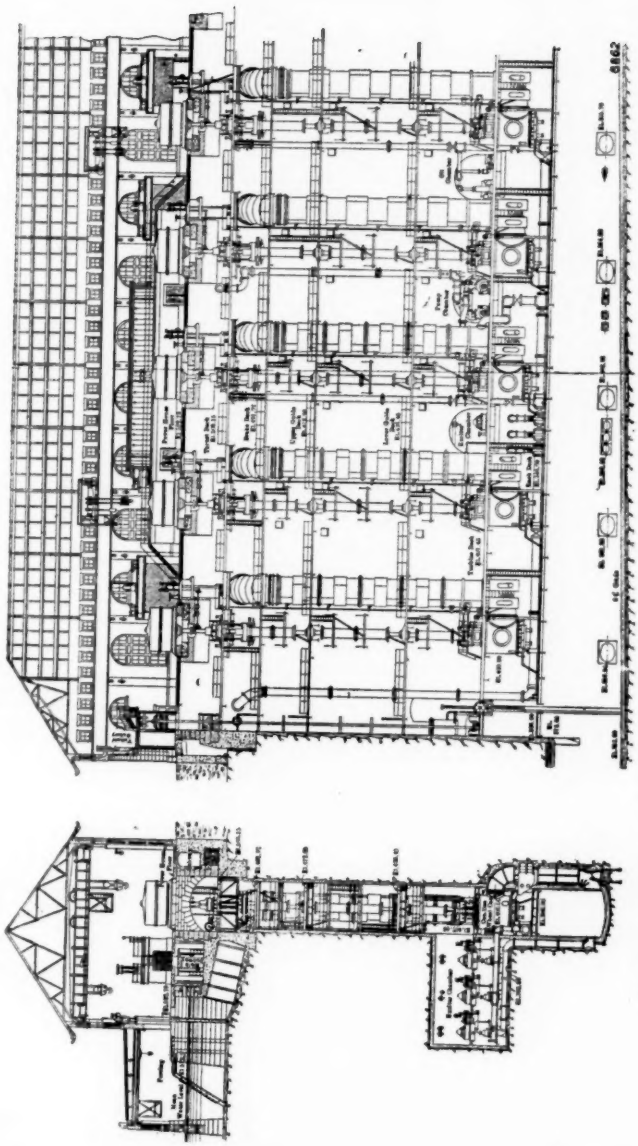

FIG. 1.—SECTIONS OF POWER HOUSE AND WHEEL-PIT, SHOWING MACHINERY INSTALLED. CANADIAN NIAGARA POWER CO.

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forebay. (*d*) A steel rack, composed of 3 by ⅜-in. bars, 1⅛ in. apart, making an angle of 60 deg. with the horizontal, extends the entire length of the forebay, thus protecting the entrances to the penstocks.

Wheel-Pit.—Although the general location of the plant predetermined the generation of the power by wheels placed in an excavation and discharging through a tunnel having an outlet below the Falls, yet the form of such an excavation was dependent on the question of vertical or horizontal shafts for the machinery. This problem was decided in favour of vertical shafts, not only because of the greater convenience in operation afforded by having the generators on the power house floor, but because of the practical difficulties which would be encountered in arranging the machinery for easy dismantling if horizontal shafts were used. On account of these and other considerations, it was determined to use units with vertical shafts, the spacing of the units to be 48ft. The length of the wheel-pit was fixed at 566·84 ft. and the depth, to meet the tunnel grade, 171·1 ft. below the power-house floor, the width of rock excavation being 20·5 ft.

Fig. 1 shows the completed wheel-pit, with the machinery installed, and also the lines of excavation and the brick lining walls. Four chambers, 21 ft. wide, 50 ft. long, and of varying depths, were cut into the east side of the wheel-pit to provide for the installation of auxiliary machinery, such chambers being lined with brickwork 2 ft. thick.

The bottom of the wheel-pit was given a grade of 3 per cent. in order to equalise as fully as possible the velocities in the discharge water in the wheel-pit.

The upper part of the wheel pit, to a depth of from 25 to 30 ft. below the power-house floor, is lined with a retaining wall, the upper portion of which is faced with first-class, close-pointed masonry, while the lower part is of concrete. In connection with this upper lining wall were built the arches, 24ft. in length, supporting the stationary part of the generator. These may be seen in Fig 1.

Tunnel.—The tunnel is 2,164 ft. long, of horseshoe form cross section, 25 ft. high and 18·84 ft. wide at its widest point.

The gradient of the tunnel is 7 ft. per 1,000, except near the portal, where it falls 11·2 ft. in 103·08 ft., a reverse vertical curve, with a radius of 248 ft. at the springing line, being used. The greater part of this portion is lined with granite blocks 2 ft. thick to withstand the increased velocity of the water.

Much of the material excavated from the tunnel, as well as the larger quantities removed from the canal and wheel-pit, enabled the Park Commissioners to consummate a plan which they had formed many years before and had partly carried out in 1895, namely, the reclamation of a large area of unsightly rock surface at the west end of the Horseshoe Falls left practically bare by the rapid recession of the apex of the Falls and the consequent deepening of the central portion of the channel.* The filling of this area made possible the construction of walks and driveways from which visitors may obtain a close view of the most impressive portion of the cataract, and has greatly improved the scenic conditions at this point.

Power-House.—The power-house superstructure is a fireproof building, 110 ft. 10 in. wide, now completed for a length of 296 ft., to cover five units, its ultimate length being 588 ft. 8 in. The main power-room, covering the generators and switchboard, has a clear width of 68 ft. 2 in. and a height of 64 ft. 1 in. to the ridge. Along the east side of the building is the enclosed forebay, having a clear width of 36 ft. 6 in.

The floor in certain sections of the power-house was designed to support the heaviest parts of the machinery arranged in that manner which, of all practical ones, would give the most severe stresses in the supporting beams, the portions of the floor thus designed being marked by brass plugs.

The roof, crane girders, and head-gate girders are carried on steel columns, encased in brickwork, projecting as pilasters into the power-room. The walls are of massive character, suitable for such an installation, being from 26 to 30 in. thick.

The roof is covered with semi-glazed green tile laid directly on steel purlins. It is believed that such a form of construction is somewhat novel for a building of such importance, and where the consequences of leakage would be so serious. The results have been entirely satisfactory, no leakage or condensation taking place since construction ceased.

As shown in Fig. 1, the space below the power-house floor is completely utilised. A subway, 5 ft. in width between the wheel-pit walls and the west foundation wall, provides for piping and auxiliary wiring and cables, and the main subway on the opposite side of the wheel-pit contains the bus-bar structure and the dynamos and feeder cables. On the east side of the subway, chambers between the inlets contain lockers for the employees, heating and lighting transformers, oil-storage tanks, etc.

The power-house is provided with two 50-ton electric travelling cranes, and these are used together for handling the armature of the generator; this piece weighing about 90 tons. A 5-ton 3-motor electric travelling crane in the forebay is used principally for moving racks and stop-logs.

A hydraulic passenger elevator furnishes

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* The erroneous statement has been widely published that the crest line of the Canadian Falls has been shortened to the extent of several hundred feet by the power companies for their own benefit. The facts are as stated above. All the excavated material deposited within the park limits was placed in accordance with the specific directions of the Park Commissioners. No power company has been affected, beneficially or otherwise, by the filling-in of the portion of the river-bed from which the water had receded years before the power development began.

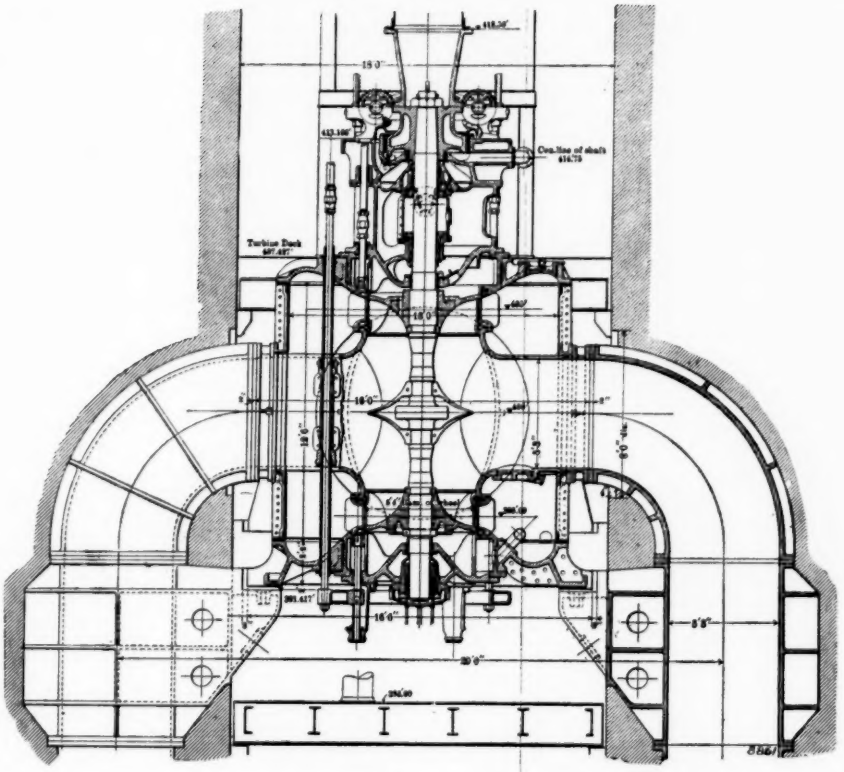

FIG. 2.—DETAILS OF TURBINE, CANADIAN NIAGARA POWER CO.

access from the power-house to the several decks of the wheel-pit.

A structure, 127 ft. 6 in. by 17 ft. 6 in. by 11 ft. 7½ in., is built on the power-house floor to enclose the oil switches and to provide a foundation for the switchboards as well as a working platform for the electricians-in-charge. Ducts are placed in the walls for carrying the wiring to the switches. Ducts beneath the power-house floor provide for carrying all cables between the generators and the main subway, as well as between the several subways and chambers. Pipes in the pilasters of the building carry wiring to the lights, so that all cables and wiring are concealed.

Hydraulic Machinery.—The size and gradient of the tunnel having been determined, thus fixing its elevation at the point of juncture with the wheel-pit, the elevation of the working floor beneath the turbines, called the "rack deck," was selected so as to keep it above the surface of the tail-race water under all ordinary hydraulic conditions. At an early stage it was determined that draught tubes were to be used, and, in order to keep their outlets submerged when only a small number of units were in operation, a controlling gate was installed. This gate is operated by an 85-h.p. motor. The installation and correct alignment of a gate of this size, 23 ft. by 20 ft. 5 in., with a travel of 25 ft., at the bottom of an excavation 171 ft. deep, was quite difficult, but the gate has operated successfully. Although the gate is used daily in the operation of the plant, it has been found that the form and size of the draught-tubes are such that the vacuum is not broken if the outlets are uncovered, and, although the vacuum is not as steady as at other times, no interference with the usual operation would occur if the gate were temporarily inoperative.

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The general form and arrangement both of the main and auxiliary machinery are shown in Fig. 1. The water, after passing through the racks, enters masonry inlets, 18 ft. wide, through which it flows to the penstock mouthpieces. Descending into the wheel-pit through the penstock, the water passes into the wheel-case and thence through the twin wheels to a common central draught-chest from which it reaches the tail-race through draught-tubes built into each of the wheel-pit walls.

The total available head, under the usual conditions, will be 136 ft., but, with low water and peak loads, it may be reduced to 130·5 ft.

Careful consideration was given to the question of the speed of the units, and, for both hydraulic and electrical reasons, the machines were designed for 250 r.p.m.

The penstocks were made of cast-iron segments 10 ft. 2 in. in diameter. The resulting velocity will be 11·2 ft. per sec. when 10,000 h.p. is being developed under minimum head.

The lower penstock elbows are interesting as examples of large steel castings, the over-all dimensions of the complete elbow being 12 ft. 11 in. by 13 ft. 6⅝ in. by 16 ft. 6 in.

Considerable thought was given to the design of the draught tubes, as the conditions affecting their installation were peculiar. As finally made, they are uniformly 5 ft. 3 in. diameter, and when the unit is running under full load the velocity therein is about 21 ft. per sec. While this high velocity represents a material loss, not only in velocity head but also in friction, it is useful in imparting to the discharge water a part of the velocity required in the tunnel, the lower section of the draught tubes discharging at an angle of 45 deg. with the axis of the wheel-pit. Fig. 1 shows the form of the tubes and the method of supporting the wheel-case on the section immediately below the upper elbow.

Two Francis turbines are used, the water passing from the steel-plate wheel case to a common cast-iron draft chest. The regulation is effected by cylinder gates operated by a system of racks and gears actuated by a rod from the governor which passes down the pit along the penstock. The efficiency at partial gate openings might be greater with a regulation by movable vanes in the distributors, yet the practical difficulties of their operation in the space available would be great. Experience has shown that the accurate fit of the cylinder valves between the distributors and the turbine and their accurate guidance is a matter of the greatest importance. Clearances at these points cause vibrations which are most injurious.

The turbine shaft, like all other portions of the solid shafting, is made from oil-tempered nickel steel having an elastic limit of 65,000 lb. per sq. in. It is guided by three bearings, two above the upper runner and one below the lower runner, the shaft being divided into two parts for convenience in dismantling. Difficult features of the design are: to prevent the escape of the oil used for lubricating these bearings, and to keep water out of the oil. This is done by the use of cup-leather packings, it having been found that hemp packings scored the shafts.

Special interest attaches to the method of supporting the revolving parts of the hydraulic and electrical machinery, weighing 240,000 lbs. moving at the rate of 250 r.p.m. From Fig. 2 it will be seen that by means of an opening in the wheel-case base ring, surmounted by an elbow, water passes to the space below the lower runner, which is thus under the full wheel-case pressure. The space above the upper runner communicates freely with the draught chest through the openings shown, and pressure, therefore, cannot accumulate there from the water which leaks past the upper circular valve. With this arrangement, the upward pressure beneath the lower runner is not balanced by a pressure above the upper runner. The upward pressure thus provided for is not sufficient to support the entire rotating weight, and additional balancing pressure is effected by the piston shown immediately above the upper bearing. A 12-in. pipe is carried down the wheel-pit to connect by two 6-in. pipes with opposite sides of the space beneath the piston, an emergency connection being made with the wheel-case. The outside diameter of the piston being 3 ft. 10 in. and the inside diameter 21½ in., there is thus an area of 1,298 sq. in. subjected to the pressure of the water, which is about 43 lb. per sq. in.

It would be possible, of course, to design the balancing piston so that, in conjunction with the pressure under the lower runner, it would exactly balance the weight of the revolving parts under a given condition of load on the unit and elevation of tail-water; but such conditions are changing, and provision must be made, not only to take care of their variations, but also to provide for failure of the water pressure under either the balancing piston or the lower wheel, or under both. This has been by the installation of an oil-step bearing, placed at the deck immediately below the power-house floor, and supported by the heavy girders shown on Fig. 1. It consists of two discs of charcoal iron, one stationary and the other secured to the shaft, oil being forced between these discs by a special pump driven from the main shaft. As the pump supplying the oil is of the three-plunger pattern, moving at constant speed, the bearing acts automatically, the pressure of the oil corresponding to the weight to be lifted. These bearings have never been called upon to carry the entire rotating weight, but it is believed that they would do so successfully. Unit No. 1 was operated for some time with the balancing piston removed, and Units Nos. 4 and 5 were each run for one hour with the balancing piston pressure cut down to 5 lb., the rise in the temperature of the oil being only 4°, one gallon of fresh oil per minute being admitted to the suction tank of the pump. The pressure of the oil rose to only 110 lbs. per

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sq. in., although the pumps are capable of exerting a pressure of 500 lb. per sq. in.

An interesting question arose as to the maximum capacity of a prime mover, of the general design of the present units, under the conditions imposed by the existing construction of penstock elbows and draught tubes.

Six tests were made at practically full load, and the following shows the average distribution of the losses from the average total head of 134·56 ft. between head water and tail water:

	Per cent.
From forebay to bottom of upper elbow ...	8·33
In straight vertical portion of penstock ...	0·34
In lower elbow and wheel-case	11·83
Friction loss in draft-tubes	11·30
$\frac{V^2}{2g}$ in discharge water	18·95
In turbine wheel, and mechanical losses ..	49·25
	100·00

It was determined that, with turbine machinery of the present design, in conjunction with existing draught tubes and penstock mouthpieces, the maximum available power per unit on the turbine shaft would be about 11,500 h.p., and that beyond that point the losses would more than counterbalance the power contained in increased quantities of water. This, however, should not be regarded as a criticism on the present design, as it performs efficiently the work for which it was intended.

The governors for the main units are of the oil-pressure, single-acting type, patented by Escher, Wyss & Co. A compression member connected with the piston in the main cylinder extends to one end of a lever on the thrust deck, and, from the opposite end of the lever, a rod passes to the rack and pinions operating the cylinder gates, a weight of about 7,000 lb. being attached to the lower end of the rod. The gates close by gravity and open by the oil pressure. As showing the accuracy of regulation which obtains, the following extract from the contract with the manufacturer may be of interest:—

Changes in load.	Corresponding maximum speed variation.	Time of perturbations in seconds.
25 per cent.	2 per cent.	25
50 „	4 „	35
75 „	6 „	50
100 „	8 „	60

In the northerly chamber, at the level of the turbine deck, are installed three exciter units, each consisting of a 200-kw., 125 volt vertical-shaft, direct-current dynamo, driven at 600 rev. per min. by a turbine of the Francis type. The direct current thus generated is used, not only for exciting purposes, but also for operating motors and for furnishing light and heat.

Electrical Installation.—The generators are of the revolving, internal-field type, furnishing 25-cycle three-phase current at 12,000 volts. The machines are rated at a capacity of 10,000 e.h.p., but they are capable of a large over-load. Exciting current at 125 volts is supplied to the collector rings immediately below the upper shaft coupling. From the generator the current is carried in ducts to the main subway and thence to double-throw selector oil-switches, through which it passes to the four sets of bus-bars. The oil-switches, in addition to their automatic action in cases of short circuits, are operated by relay switches located on panels on the floor of the switchboard gallery.

All the cables are carried singly upon grooved electrose supports secured to horizontal channels. In case of a short circuit, it is believed that no arc will form between the cables; and their thorough separation thus minimises the possibility of trouble resulting from burn-outs. The cables are carried to a connection with the two main conduits, namely, that leading to the upper steel arch bridge across the Niagara River, and that leading to the transformer-house at the top of the escarpment. The former group interconnects the plants of the Canadian Niagara Power Co. and the Niagara Falls Power Co.

The transformer station is divided longitudinally into three sections. The east side contains the low-tension switches and air-cooled 11,000—2,200 volt-transformers; the central bay contains the main, oil-insulated, water-cooled transformers: and in the west compartment are the high-tension switches and bus-bars. By this arrangement of the building the several classes of apparatus are completely separated from each other, and, as the building is absolutely fireproof, continuity of service is secured as well as possible. The main transformers, by which the voltage is raised for overhead transmission to Buffalo, are wound so as to be capable, with an 11,000-volt primary current, of giving a secondary current of either 22,000, 33,000, 38,000, or 57,300 volts.

Transmission Lines.—The output of the transformer station is now used principally in supplying 22,000-volt current to the Cataract Power and Conduit Co. at a new station built near their local centre in Buffalo. The Canadian transmission lines are built on a private right of way, 30 ft. in width, from the transformer station to Terminal House B in Buffalo, a distance of sixteeen miles. One pole line containing two circuits is in service, and the building of a second line is now in progress. Each circuit has a rated capacity of 12,500 h.p., or a total transmitting capacity over the right of way of 50,000 h.p. at 22,000 volts.

Both the pole line and the overhead crossing contain several novel features. The insulators are electrose and the conductors are 500,000 circ. mils, aluminium, with thirty-seven strands.

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RAILWAY NOTES

Working of Nationalised Railways in Japan.

A recent issue of the *Board of Trade Journal* states that the working of the railways since their nationalisation has given satisfactory results. The length of lines open in 1904—5 was 4,693 miles, and in 1907—8 exceeded 5,000 miles; the number of passengers carried rose from 104 millions in 1904—15 to 125 millions in 1906—7; and the volume of goods traffic rose from 19 million tons in 1904—5 to 24 million tons in 1906—7. Through the improvement of the means of transportation and of the general economic condition, the railway profits have increased beyond expectation. According to the estimates formed when the nationalisation scheme was first considered, it was anticipated that, during the first two financial years, the profit would be insufficient to meet the interest upon the debts incurred on account of the purchase; but in the financial year 1906—7 the profit was £1,709,780 as against the estimate of £1,586,224, and in the year 1907—8 it is believed that the actual receipts will exceed the estimated amount, which is £3,208,287, so that the profit from the purchased railways will be more than sufficient to pay the interest. The railway profit for the financial year 1908—9 is estimated at £3,796,565, of which that expected from the purchased railways is put at £2,544,168, and, as the total amount of interest payable on the purchase prices and debentures taken over from the companies is £2,463,437, there will be, after the interest is paid, a surplus of £80,730.

A Simple Track Inspecting and Recording Machine.

Several attempts have been made to devise an apparatus which could be applied to an inspection car to indicate approximately the condition of the road in respect of variations in gauge and level, but owing either to their intricacy, costliness or unreliability when the running speed varied, little progress has been made with its introduction. What appears, however, to be a reliable and inexpensive machine suitable for use by divisional engineers is described in a recent issue of the *Engineering News*. It is the invention of a roadmaster on the Northern Pacific Railroad, on which line it is in actual service. The machine, which runs on three wheels, is coupled behind an inspection or hand car, and can be run at any speed up to about 12 m.p.h. It weighs 200 lbs. On a continuous roll of paper it draws a diagram showing the condition of the road as to surface, and if the rails are level on tangents, and have the proper super-elevation, on curves. It also records low joints (showing the amount of the depression), and all variations from the proper gauge. On the machine is a wheel exactly 3 ft. in circumference, which is connected to a cyclometer and is used in making measurements. The lineal scale of the diagram is 200 ft. to the inch, or 26·41 ft. to the mile; the natural scale is used for the gauge, and half the natural scale for the elevation. Figs. 1 and 2 show portions of diagrams made on the Northern Pacific Railway, and representing good and bad conditions of track.

The driving gear moves the paper over the roll at the rate of 26·41 ins. per mile of track. A pendulum suspended from the vertical frame moves to right or left as the wheel on either side goes higher than the other, thus moving one stylus to right or left. The paper has a heavy ruled line, upon which the stylus follows when the track is perfectly level, and marks to the right or left of this line show how much the rail is out of level or what is the amount of elevation on curves. The paper is ruled with lines ¼ in. apart. A record of the gauge is obtained by using two pieces of tubing for an axle, one piece being large enough to admit the smaller piece. Inside the large tube is placed a graduated spring which keeps the wheel flanges tight against

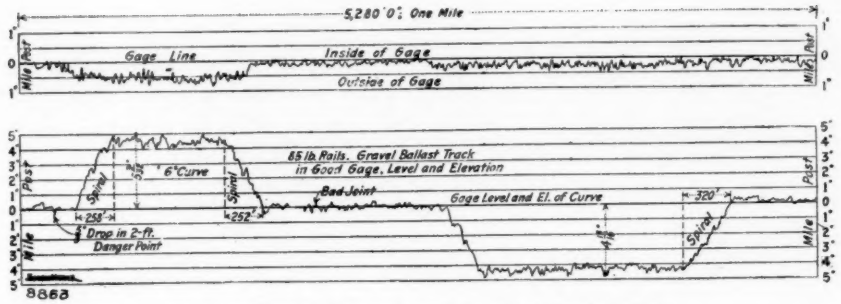

FIG. 1.—DIAGRAMS MADE ON GOOD TRACK BY THE INSPECTING MACHINE.

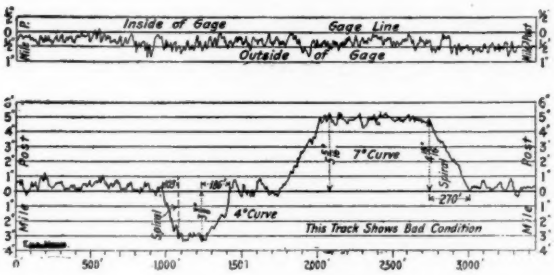

FIG. 2.—DIAGRAMS MADE ON POOR TRACK BY THE INSPECTING MACHINE.

the rails. The rod attached to the loose end of the axle carries the stylus, which comes in contact with the paper and marks every change. A heavy line ruled on the paper shows where the stylus should be at standard gauge. The distance that the diagram drawn by the stylus varies from this line shows how much too wide or narrow the gauge is at any point.

A carbon belt runs outside all the rollers and directly under the stylus in contact with the paper. Thus the use of ink or pencils is obviated. A corrugated roller which presses on the carbon belt leaves an impression on the paper which comes out from under the roller ruled. This obviates the necessity of having paper specially ruled for the purpose. Any paper that is of the correct width can be used in this machine, or a transparent paper can be furnished if required for blue printing. The rollers carry sufficient paper for 50 miles of line.

A study of the diagram will enable the engineer to ascertain the condition of the track as a whole, and to see the several points which need attention. Blue prints can then be made, and a print of each section sent to the section foreman, with notes and instructions as to bad places and the necessary work of improvement.

Size and Capacity of Safety Valves for Locomotive Boilers.*

During the past few months this committee has been collecting data from the various railway companies in order to arrive at some definite conclusions regarding existing practices. A letter of inquiry was also addressed to the various valve manufacturers and to the locomotive builders, to whom the committee feels grateful for their assistance and the information they have so willingly furnished. The replies from some twenty railways show that the safety valves now in use are of sufficient capacity, and on these reports the committee has based the calculations that are to follow. The records from twelve important roads show that the lift of the valves varies from $\frac{1}{32}$ to $\frac{1}{10}$ in.

Taking the mean effective area of opening

* Abstract from the Report of a Committee to the American Railway Master Mechanics' Association.

in square inches per 500 sq. ft. of heating surface, based on existing average practice of twelve railroads, we have developed the following empirical formula—

$$A = \frac{0{\cdot}10266\ H.S.}{P};$$

where A equals the effective area of opening of the valve in square inches; H.S. equals the heating surface of boiler in square feet; and P equals the absolute pressure, equals gauge pressure plus 15 lbs. The formula is based on an evaporation of 5·28 lbs. of water per square foot of heating surface per hour, and we recommend it for use in the application of safety valves until such time as it is shown to be in error, or, upon future investigation, a better one shall have been devised.

The valves of nine prominent valve manufacturers show a lift ranging from 0·03 up to 0·15 in.; taking an average of these lifts (0·087 in.) and working out the values for typical modern shunting, goods and passenger locomotives, we give the following tabulation illustrating the application of the empirical formula—

Type of locomotive.	Service.	Heating surface, sq. ft.	Gauge pressure.	No. and size of valves.
Consolidation ...	Freight ...	3,200	200	3, 3½ in.
6-wheel switch..	Yard ...	1,900	200	2, 3½ in.
Pacific	Passenger .	3,500	200	3, 3½ in.

In this tabulation it will be seen that the number and size of safety valves for the Pacific and Consolidation type locomotives are the same. This is so because the committee deems it advisable, and would recommend that the railway companies adopt one standard size of safety valve for all their locomotives, and not have, say, two 3 in. and one 3½ in. valve on the same locomotive. We feel that the adoption by railroads of one size of safety valve for all locomotives will bring about a uniformity that is much to be desired. The valves shown in the tabulation are assumed to have 45 deg. seats.

In all these deductions one must bear in mind that the committee has assumed a rate of evaporation based on the capacity of discharge of the safety valves now in use, and since a vast majority of the railways are experiencing no difficulty with safety valves, this seems to be a reasonable assumption.

Single-Phase Electric Railways in Europe.

The increasing application of this system of railway operation makes any figures in connection with it of considerable interest. The *Electrician* recently gave some information regarding its development in Europe, from which it appears that there are 19 lines totalling 415·5 miles. As several of the lines are of a more or less experimental description, details as regards motor car or locomotive equipment and motor horse-power are somewhat meagre, though the total is in excess of 47,120 h.p. The following is the table given by our contemporary:—

Name of line.	Length in miles.	Frequency.	Line voltage.	Number of motor-cars with h.p. on each.	Number of locomotives with h.p. on each.	Date of opening.
Bergamo-Valle Brembana...	19·0	25	6,000	—	5—300	December, 1907.
Blankenese-Ohlsdorf ...	41·5	25 25	6,600 6,000	6—250 51—345	—	January, 1907.
Borniage	13·0	40	600	20—80	—	April, 1905.
Compagnie Générale Parisienne de Tramways ...	1·0	—	500	2—50	—	Experimental.
Murnau-Ober-Ammergau ...	15·0	16	5,000	4—160	—	January, 1905.
Spindlersfeld	26·0	25	6,000	2—200	—	August, 1903.
Prussian State Railway ...	1·5	25	6,600	—	—	Experimental.
Rome-Civata-Castellana ...	34·0	25	6,000	15—80	—	March, 1907.
Stubaital	11·75	42	2,500	4—160	—	August, 1904.
Swedish State Railways ..	12·0	25	6,000 5,000—20,000 6,000—20,000	— —	2—200 1—330 1—300	Experimental.
Vienna-Baden	17·0	15	10,000 A.C. 500 D.C.	13—100	—	January, 1907.
Locarno-Bignasco	17·0	20	5,500	4—160	—	September, 1907.
Seebach-Wettingen	12·0	15	15,000	—	2—400 1—225	December, 1907.
Midland Railway	16·6	25	6,600	3—350	—	April, 1908.
Brighton Line	25·0	25	6,700	16—500	—	
Parma Provincial	26·5	25	4,000	17—120 1—60	—	Not yet open for traffic.
Rotterdam-Hague	45·0	25	10,000	20—350	—	
St. Polton-Mariazell ..	66·5	25	6,000	—	23—350	
Tergnier-Anizy	20·0	25	3,300	—	—	

ELECTRICAL PROGRESS

By ANDREW STEWART, A.M.I.E.E.

Some Features of Recent Railway Electrification Schemes.

As we have indicated at one time and another, the single-phase system of electric traction seems likely to enjoy a period of popularity which, if it does not firmly establish it as the standard system of electric traction, will place it in a position from which any other system will dislodge it with difficulty.

It is evident that despite the disadvantages from which the motor suffers on account of its weight, its comparatively low cost for overhead equipment and sub-stations, as well as the simplicity of the outside work, justifies engineers in giving it a thorough trial. These so-called trials, conducted as they are on isolated scraps of line, may not always solve many railway electrification problems, but they give the system the footing that any new system requires in order to establish it as more or less fashionable, which is almost all that is necessary to achieve success in many directions.

It is, of course, recognised among engineers, just as among other and more impressionable sections of humanity, that one's neighbours need only be doing something to induce others to do likewise, and if a railway, or a bit of one, is electrically equipped with single-phase traction, there is a strong tendency for others to adopt a similar equipment.

Our intention this month is to review the practice of some recently equipped electric railways, particularly the overhead equipment, regarding which engineers in general find it more difficult to obtain information than about the mechanical parts of the equipment.

A line possessing many features of interest, especially as the constructional details of the overhead equipment embodies many desirable features, particularly simplicity and adaptability, is the recently-completed narrow-gauge line between Locarno and Bignasco in Switzerland. It was equipped by the Oerlikon Company, whose universal collector is illustrated (Fig. 1). This collector consists of a bent steel tube with a replaceable strip of brass let into it to form the contact with the overhead or side conductor.

A helical spring causes the collector to sweep through the arc indicated by the broken line, thus enabling it to collect current from the conductor, whether it be immediately over the carriage or, as may be in the case indicated, *i.e.*, to one side of the track.

It is, of course, very convenient to have

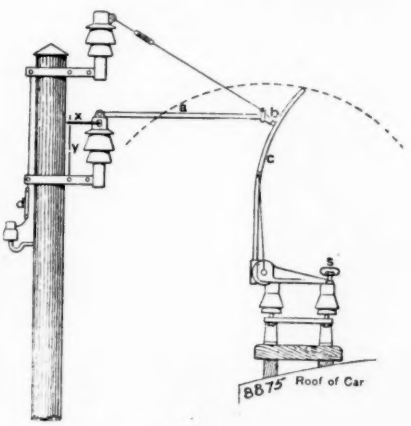

FIG. 1.

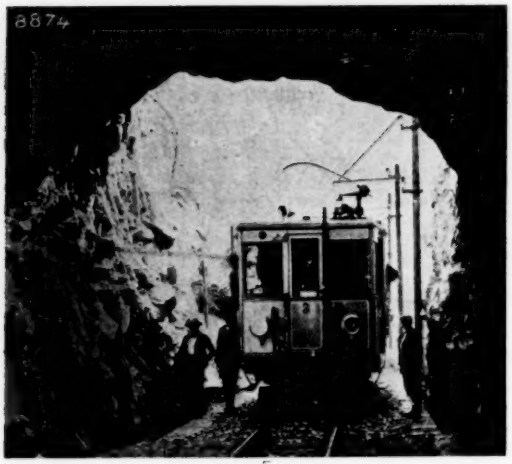

FIG. 2.

the conductor freed from the limitations which the more orthodox types of collector impose, for a side conductor permits of comparatively cheap construction under bridges, through tunnels, and in cuttings where the sides are rock, or walled.

The helical spring already mentioned maintains the pressure on the collector arm practically constant through the entire range of its movement, the pressure against the conductor being about 6½ lbs. The collector can be withdrawn from contact with the overhead wire and turned right over till it rests on the spring S (Fig. 1) by means of a small wheel in the motorman's cab.

A car showing the relative positions of conductor and collector is shown in Fig. 2, while Fig. 3 shows the actual collector with its insulators. The pressure used on this line is 5,000 volts, and the periodicity 20 cycles per second.

The trolley wire, it may be remarked, is carried at a height of 14½ to 15½ ft. above the rail level. At crossings and stations this height is raised to 17½ ft., while it is about 14 ft. 3 in. above the rail level in tunnels.

Another line using, for part of its length, the Oerlikon universal trolley just described, is the Seebach - Wettingen line, though a considerable portion has a double catenary suspension system just like the recently-electrified Heysham Morecambe section of Midland Railway, which will be noticed later.

As an example of the care taken to protect the public from the 6,000 volt alternating current (Fig. 4), which shows a footbridge over the line at Seebach, is interesting. Over the top of the bridge is a framework which carries the catenary suspension wires, roofs being placed over the bridge to prevent injury should a suspension wire become detached. Underneath the footway, just over the 6,000-volt line wires, wide guard boards are placed, and there is a steel frame which supports a wire netting cage, thus eliminating all chance of any passenger making contact with a live wire, even if the attempt were made. In the background of this illustration can be seen the steel bridges which support the wires under the usual conditions. This is, of course, similar to that used on the Midland Railway, and to what is being erected on the short section of the London, Brighton and South Coast Railway.

The locomotives on the Seebach-Wettingen line are equipped with the Oerlikon collectors shown (Figs. 1 and 3) and with Siemens bow collectors, which will be referred to later.

FIG. 3.

The work done on the Midland Railway is in many respects interesting. A system of catenary construction not yet described in our columns has been adopted, and as it seems more likely to prove free from breakdowns than most of the double catenary systems that have had their origin in the United States, it deserves some attention.

As the line passes under several

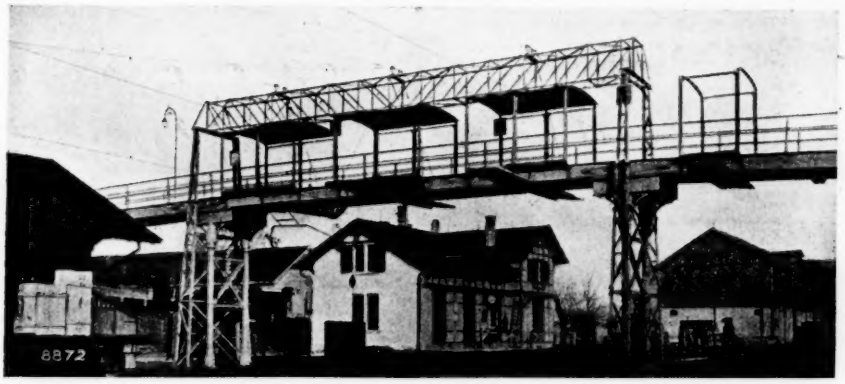

FIG. 4.—BRIDGE ON THE SEEBACH-WETTINGEN LINE.

bridges, it has been necessary to adopt some arrangement of collector which permits not only deviations from the centre of the track, but considerable variations in the height from the rail level; for instance, the conductor is in most places about 18½ ft. above the rail level, but it falls to 13½ ft. in tunnels. Fig. 5 shows a wide-span bridge supporting the overhead work, from which it will be seen that there is a single supporting catenary wire, which supports an auxiliary catenary wire; from the latter, supported by short loops about 4 in. long, hangs the live contact wire. The short loops just mentioned are movable on the auxiliary catenary wire, and the conductor is of figure 8 section, the upper half of which is gripped by the loop in the manner indicated by Fig. 6. Although there appears to be only one catenary wire there are really two, which are run together, except when they approach the insulators seen

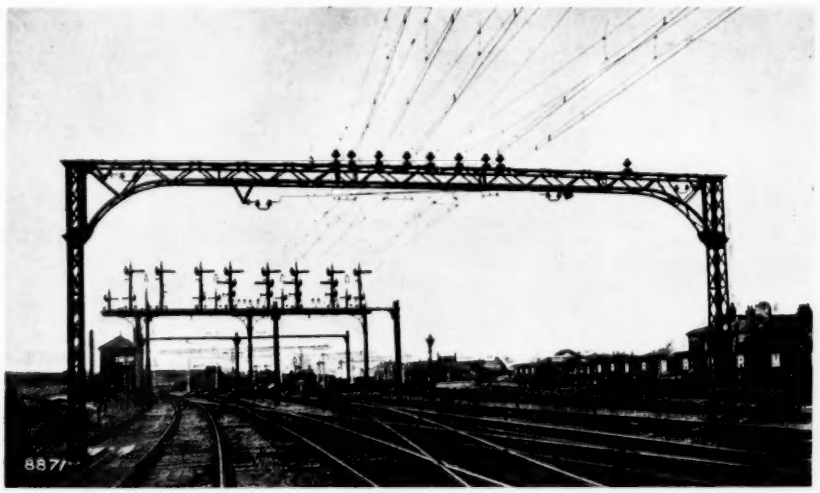

FIG. 5.—OVERHEAD WORK ON STEEL BRIDGE.

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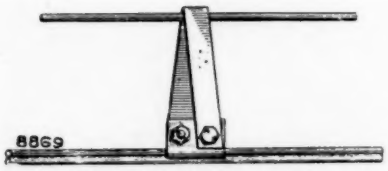

FIG. 6.—LOOP CLIP FOR CONDUCTOR.

on the top of the bridge. They then divide and pass, one on each side of the insulator, in the manner shown (Fig. 7). This division of the wires extends for about 3 ft. on either side of the insulator, and permits the wire to move this amount, which prevents overstraining from any cause.

One of the troubles, inevitable with single-phase lines, is that electrostatic induction takes place between the high potential overhead conductor and any telephone or telegraph lines in the immediate vicinity, which disturbs these circuits. This is especially so with telephone circuits, which are easily disturbed. To eliminate this, a separate steel cable is run along the line, carried on one side of the supporting bridges so that it comes between the live conductors and the telephone and telegraph circuit. This cable is earthed about every half mile through the lightning arrester earth plates, and the electrostatic action of the main wires seems to spend itself on this wire, which thus acts as a kind of electro-

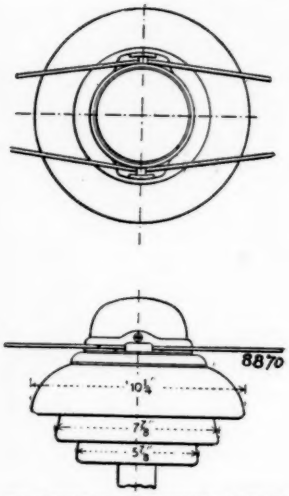

FIG. 7.—INSULATORS FOR SUPPORTING CATENARY WIRE.

static screen for the neighbouring circuits. It should be observed that expensive lattice girder supporting bridges, such as illustrated, have only been erected where the span is too great to allow of cheaper construction.

Where the latter is possible, for instance, over a double line of rails, creosoted wood poles supporting an angle iron bridge, which in turn carries the conductors, is employed. The insulator supports are attached in the manner shown (Fig. 8).

The cars having Siemens equipment have Siemens bow trolley collectors, while the Westinghouse car (Fig. 9) has a pantagraph collector.

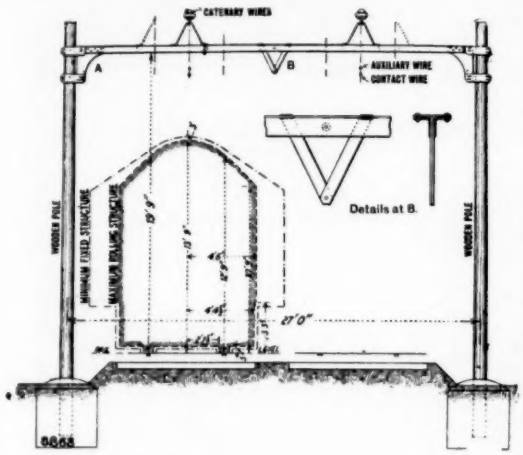

FIG. 8.—DETAILS OF POLE CONSTRUCTION OVER DOUBLE LINE.

A Belgian Steel Works Operated from a Public Electric Supply.

Most steel works that adopt electric driving obtain current from their own generating station. The steel works at Tilleur, in Belgium, are an exception, inasmuch as, though they have for some years had a small generating station supplying current for electric cranes and lighting, they decided, when further extensions of the use of electric power became imperative,

to take all their energy from the local power station near Liège. They intend, however, as soon as the use of electric power has been further developed, to erect a large blast furnace gas-engine plant of their own.

At present about 1,800 h.p. is utilised, of which about 400 h.p. was available from the old steam-driven direct-current plant, which has been retained to drive the electric lighting. The steam engines have, however, been disconnected, and alternate-current motors, working from the 6,300-volt public supply, have been coupled to these direct-current generators. All other motors are worked from the public supply, which is, however, transformed from the pressure already mentioned to 500 volts, at which pressure it is distributed through the works. The old generating station has been used to house the transformers, and feeders radiate from this point to five centres of distribution through the works, where each point feeds about seventy motors.

All the punching, shearing, and straightening machines, as well as the workshop tools, are now electrically operated.

Motors are generally fixed in an elevated position, the chief object being to avoid insulation troubles due to oxide of iron, which is always present; it appears that above a certain height from the floor the air is quite free from dust laden with oxide of iron

Another point has been the adoption of motors having what is called chimney ventilation. Totally enclosed motors, by reason of their small radiating surface and the necessity for larger frames, are expensive, yet some measure of complete enclosure is essential. A satisfactory compromise has been made by perforating the base of the motor, and casting the top of the frame with openings that are covered by a hood. The convection currents of air due to the rise in the internal temperature of the machine cause an upward current of air proportional to the rise in temperature, so that the cooling effect is considerable.

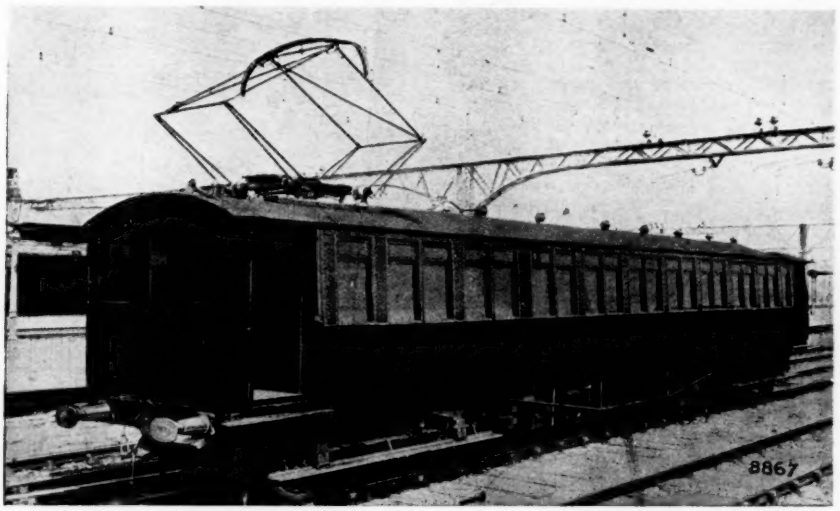

FIG. 9.—WESTINGHOUSE CAR WITH PANTAGRAPH COLLECTOR, MIDLAND RAILWAY.

Such an arrangement enables smaller and cheaper enclosed motors to be used, and while some English firms make such machines, it does not appear that works owners generally are aware of the advantages of this type.

It should be noted that tests made at this works showed that small steam engines used as much as 66 lbs. of steam per i.h.p., while radiation and leakages from the steam pipes accounted for nearly as much again.

Electric Railway Developments on the Continent.

We learn that the Baden Government have decided to adopt electric traction on

the State railways. In anticipation of considerable developments the Government have refused to allow private enterprise to develop the water power on the Rhine at Säckingen, as it will probably be required in connection with the developments indicated. About 500,000 h.p. in water power is stated to be available in Baden, though only 32,000 h.p. is necessary to work all the State lines in the province.

It is intended to start electrifying the Salzburg-Berchtesgaden line in Bavaria, using water power from a new power station at Saalach. In addition two new lines are projected, between Garmisch and Mittenwald, and between the former town and Grisen. These will be equipped electrically, two new water-power stations being built for this purpose. The Bavarian Government propose, as soon as these lines are complete, to proceed with the conversion of other lines to electric traction.

SEVENTH WORKSHOP PRACTICE COMPETITION.

AWARD OF PRIZES.

The four prizes offered for the seventh competition, which closed on September 5th last, have been awarded as follows :—

PRIZE OF £4.

E. MITCHELL,
Burdett Road,
Bow, E.

PRIZE OF £1

to each of the following :—

J. R. MAIDENS, Commercial Road, Grantham.

J. T. PAGON, Garnett Avenue, Kirkdale.

S. WALTERS, Kirkhouse Green, Doncaster.

The prize articles will be printed in a subsequent number.

MANUFACTURERS' NOTES.

Mr. Tom Walker has severed his long business connection with his brother's firm—MESSRS. JAMES WALKER & Co., Poplar, makers of the "Lion" Packing—to associate himself with MESSRS. RONALD TRIST & Co., Lloyd's Avenue, E.C.—THE ECCO BATTERY AND ELECTRICAL Co. have built and equipped a factory at Bow Common Lane, E.C.—MESSRS. CONRAD LAUER & Co. have started business as boiler and steam pipe coverers at 65, Euston Road, N.W.—THE FR. GEBAUER Co. have opened a branch in Eldon Street, E.C., and appointed as their managing agents Messrs. H. F. Crohn & Co. Messrs. Gebauer are manufacturers of high and low pressure centrifugal pumps, mining machinery, &c.—Mr. C. A. G. BROWNE, whose advertising agency has been carried on at Wych Street, Strand, for many years past, has removed to new offices at No. 20, Wellington Street, Strand, W.C. The telephone number 2345, and the telegraphic address "Reiterate," remain unchanged.

BOOKS RECEIVED.

CRANES. Their Construction, Mechanical Equipment and Working, by Anton Böttcher, translated and supplemented with English, American and Continental Practice by A. TOLHAUSEN. (London: Archibald Constable & Co., Ltd., 10, Orange St.) Pp. 510 + ix. with lxiii. Plates. Price 42*s.* net.

ELECTRICAL ENGINEER'S POCKET BOOK, by HORATIO A. FOSTER, M.A.I.E.E. Fifth Edition. (London: A. Constable & Co., 10, Orange St.) 1908. Price 21*s.* net,

ELECTRIC FURNACES. The production of Heat from Electrical Energy and the Construction of Electric Furnaces by WILHELM BORCHERS. Translated by H. G. SOLOMON, A.M.I.E.E. (London: Longmans, Green & Co. 1908. Pp. 220 + x. Price 7*s.* 6*d.* net.

LOGARITHMS FOR BEGINNERS by CHARLES N. PICKWORTH. Second Edition. (London: Whittaker & Co., 2, White Hart St., E.C.) Price 1*s.*

THE SLIDE RULE. A Practical Manual by CHARLES N. PICKWORTH. Eleventh Edition. (Manchester: Emmott & Co., Ltd.) Price 2*s.*.

XUM

Iron, Steel and the Foundry.

Conducted by PERCY LONGMUIR, B.Met.

FOUNDRY PRACTICE.

A Study of Cupola Blast Pressures.—In a recent issue of *The Foundry* Mr. W. B. Snow deals with some aspects of cupola blast pressures. The rate of melting in an average cupola depends upon the volume of air supplied; this in turn depends upon the pressure of the blast, while the intensity of blast with a given blower is in part dependent upon the freedom of discharge through the cupola. This introduces the unknown variable in cupola practice, for there is no fixed method of charging, nor is there uniformity in the size of fuel, pig iron or scrap. As a result there is a great range in blast pressure requirements between the deep, closely-packed cupola and the shallow cupola with very open charges. The variety in conditions with certain size cupolas is well shown in the following table, which gives a few records taken at random:—

Diameter of cupola lining "Inches."	Tons per hour.	Pressure in ounces per sq. in.
44	6·7	12·9
44	7·3	16·4
47	8·4	17·5
49	9·07	11·8
54	7·65	13·6
54	8·75	11·0
54	9·41	12·5
54	10·2	20·75
54	10·6	20·62
60	12·35	15·3
60	14·75	16·8
60	13·8	12·6
74	13·0	8·68

A further difficulty in making comparisons lies in the uncertainty as to the method employed for obtaining the reported figures. There is opportunity for great range in the pressure, according to whether it is taken at the blower, in the pipe, or in the wind-box; whether it is the maximum, minimum or an average, and whether under any of these conditions the gauge was suitably located. Taking these features into account, a comparison of the results has led to a determination of the approximate maximum pressure required under average conditions for a given size cupola. This relation of blast pressure to cupola diameter is shown on the accompanying curve (Fig. 1). The same diagram gives a second curve showing catalogue values in pressures for given cupola diameters as published by a leading blower manufacturer.

Much has been written as to the necessity of sufficiently high pressures in order to force the air to the centre of the cupola. Diameter is only one of the factors in the creation of resistance; the height of the cupola with its contained charges presents a far greater distance through which the air must pass. The air must pass through tortuous channels, and its velocity to move a given weight must increase as the temperature is raised in the process of combustion. The operation is extremely complicated, and is one practically impossible to determine. Knowledge as to conditions within the cupola must therefore remain more or less inexact.

Another feature of the pressure curve (Fig. 1) is that it indicates modern progress in the lowering of blast pressures and the use of large cupolas and blowers to secure results previously sought by forcing those of smaller size. When a cupola is forced through the medium of high pressure the waste of power is out of all proportion to the increased capacity. The pressure determines the rate, which varies as the square root of the pressure. The curve (Fig. 2) clearly shows this fact. Here an output of 9 tons per hour has been assumed for a given cupola when operating at a minimum pressure of 10 ozs. The increase in capacity for any increase in pressure is shown by the curve up to 16 ozs., at which pressure the rate is only 11·39 tons, an increase of scarcely more than 25 per cent. in capacity corresponding to an increase of

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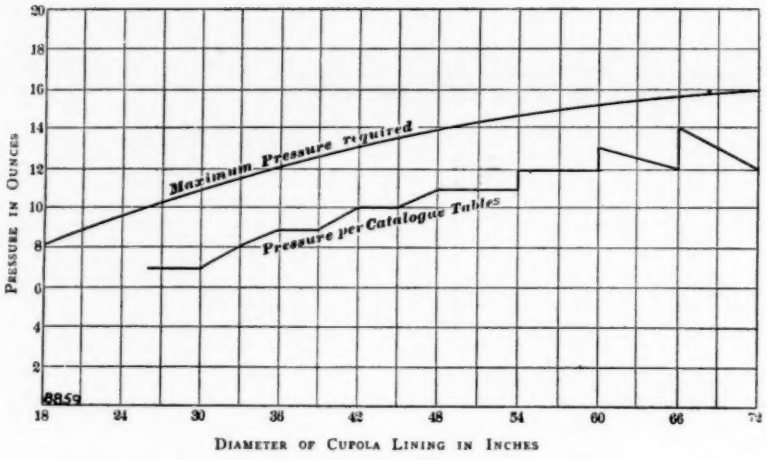

FIG. 1.—RELATIONS OF PRESSURE TO CUPOLA DIAMETERS.

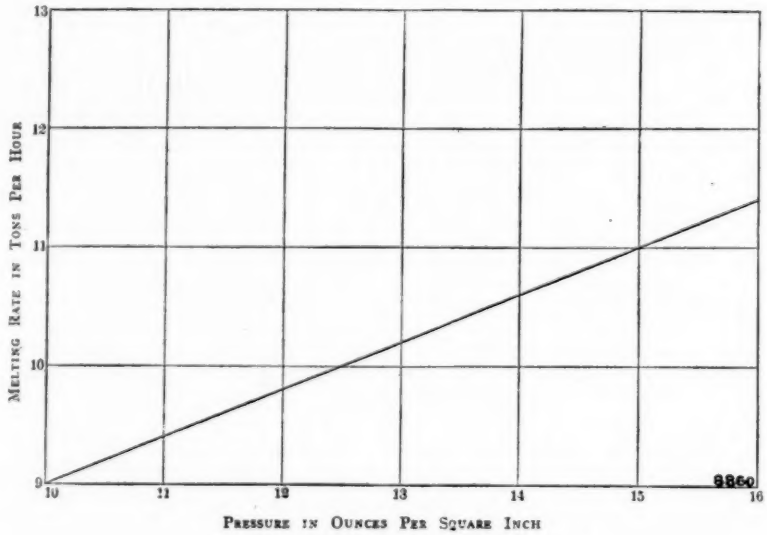

FIG. 2.—RELATION OF PRESSURE TO MELTING RATE.

60 per cent. in pressure. The rapidly decreasing efficiency of a cupola under such conditions is evident. If a short quick heat is necessary to give the greatest possible time for moulding, it had better be secured with a larger cupola, using lower pressure and requiring less power per ton of iron melted. Everything points towards the latter condition as the ideal to be sought in modern cupola practice. Not only does it insure less wear and tear upon the blower, but it materially reduces the cutting effect upon the cupola lining and improves the general efficiency of the entire equipment.

Production of Malleable Castings.—In the sixth article of this series* Dr. Moldenke discusses the work of the pattern shop, incidentally describing this shop as one of the most important departments of a malleable iron foundry. Compared with a grey iron foundry, the product of the malleable shop is very light and the number of castings made from practically every pattern enormous. Hence every refinement in pattern-making for light castings is found in this branch of the foundry industry. A modern pattern shop will be equipped with the usual wood-working machines, as well as individual wood-trimmers and other labour-saving devices for each bench, and also have every facility for metal pattern-making as part of the installation. In fact, the pattern shop becomes a foundry for white metal castings, a tool room, machine shop and model works all combined.

When developing a new article the pattern maker must practically live in the foundry. As the trial castings are made he must measure them up with the pattern, make little changes here and there, try the matter out again, both before and after annealing. These trials should be made with iron from different parts of the heat, with an endeavour to bring out new points calculated to make the moulding easier, the difficulties from shrinkage less, and swing the job so that it can be carried on without difficulty. It should be the rule never to start an order for quantities until the pattern has been thoroughly tried and all initial difficulties removed.

Contraction and shrinkage are two separate and well-understood features in malleable work. Shrinkage is understood to mean the tearing apart of the particles of iron in the interior of a larger section close to a smaller one, leaving a spongy mass, naturally weak, which is dangerous to the life of the casting. Contraction, on the other hand, is simply the reduction in size incident to the cooling of a casting from the point of solidification to ordinary temperature. This contraction in the hard casting is, roughly, $\frac{1}{4}$ in. per ft. During annealing one half of the contraction is recovered, hence the net result is that the same allowance is made as in ordinary foundry pattern practice. Contraction is not necessarily constant, and may actually vary from $\frac{3}{16}$ to $\frac{5}{16}$ in., and occasionally a little beyond even these limits. This is caused by many factors closely associated with the constitution of white iron. Thus the lower the silicon and the harder the iron the greater the contraction. Further, the more gates used or the more steel added to the mixture—silicon of the castings being the same—the greater the contraction. In the same heat the first and last iron give different contractions, depending a good deal on whether the heat was "high" or "low" in the first place. Hence the importance of careful attention on the part of the pattern shop in following up work having to pass inspection. The composition and method of melting the metal also have an effect on the anneal, and the castings may not recover the $\frac{1}{8}$ in. allowed. Repeated annealing opens up the structure to an extent quite appreciable in measuring up the finished casting. Thus the whole question is beset with difficulties, every case requiring individual treatment.

Converter on Small Open Hearth.—In this instalment Mr. W. M. Carr* enters into the question of initial outlay, assuming an average output of 12 tons of castings per day. To keep the output steadily at the quantity mentioned three converters are advised, two of which may be in operation with one as a stand-by for repairs. Actually one converter will turn out the required quantity, but three are advised in order to meet any emergency. The melting equipment will consist of one or two 40 in. cupolas capable of melting about 4 tons of a mixture of pig iron and steel scrap per hour. Blast for the converters should be generated by a positive blower delivering air at a pressure of about 4 lbs. per sq. in., at a volume of about 880 cub. ft. per min. per ton of charge treated. In addition there will be the usual fan blower to serve the cupolas. For transferring molten iron from cupola to converter and for handling the steel there

* *The Foundry.*

* The *Iron Trade Review.*

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should be an overhead crane of at least 10 tons capacity. The cost of the equipment is summed up as follows:—

Two 40 in. cupolas fully lined at $500 ...	$1,000
Three two-ton converters at $1,500 ...	4,500
One positive blower	1,000
One fan blower	150
Ladles, two-ton capacity and smaller ...	400
Approximate cost of steel-making devices...	$7,050

In addition there will be buildings, power, machinery, moulding and core-making facilities, drying ovens, sand mills, cold saws, oil storage tank, &c., bringing the total cost up to about $18,000 according to the character of the buildings and equipment.

Turning to the open-hearth plant: whilst there may be no technical objection to building a furnace as small as two tons capacity, the question of economy has to be considered. The skilled labour will be the same cost per day in any size suited to the production of castings, but the cost per ton of output will vary with the size of furnace. The size of furnace desired will depend upon demand and other conditions. Since a furnace of five tons capacity capable of yielding four heats per twenty-four hours is of convenient size, the following figures will represent approximate cost when erected in the vicinity of Pittsburg. Costs represent furnace structure, including brickwork, structural steel binding, iron castings, reversing valve, stack and all foundations, but do not include cost of charging platform.

Brickwork	$1,357
Steel stack	600
Iron castings	500
Steel binding (erected)	1,200
Bolt and washers	240
Reversing valve...	400
Foundations (excavating and concrete) ...	690
Oil tank and pump	500
Oil burners for furnace and ladle drying ...	200
Two five-ton ladles	450
Total cost of furnace ready for bottom...	$6,137

The foregoing applies to either an acid or basic structure, and if basic should be the practice selected there would be an increase of about $200 for bottom making material.

Titanium in Cast Iron.—In a paper read before the American Foundrymen's Association, Dr. Moldenke describes a series of experiments conducted on behalf of the Association with a view to determining the effect of titanium on cast iron. Two types of ferro titanium were used, one free from carbon and the other containing about 5 per cent. of carbon. Tests showed that whilst both alloys could be used, the carbon titanium alloy, by virtue of its lower melting point, is better adapted for the ladle addition in foundry work. The carbon free alloy may be used in the foundry, when large bodies of metal are to be treated and time can be given for a thorough incorporation of the material. Grey and white irons were experimented with, and in all some ninety-three tests are reported.

These tests are summarised in the following table:—

	No. of Tests.	Transverse lbs.*
Original iron, grey	9	2020
Original iron, plus 0·10 per cent. titanium	4	3100
Original iron, plus 0·10 per cent. titanium	3	3030
Original iron, plus 0·05 per cent. titanium and carbon	6	3070
Original iron, plus 0·10 per cent. titanium and carbon	6	2990
Original iron, plus 0·15 per cent. titanium and carbon	4	3190
Average		3070

Increase in strength treated iron over original = 52 per cent.

	No. of Tests.	Transverse lbs.
Original iron, white	8	2050
Original iron, plus 0·05 per cent. titanium	11	2400
Original iron, plus 0·05 per cent. titanium and carbon	9	2420
Original iron, plus 0·10 per cent. titanium and carbon	10	2400
Original iron, plus 0·15 per cent. titanium and carbon	10	2520
Average		2430

Increase in strength treated iron over original iron = 18 per cent.

The grey iron is more susceptible to the influence of titanium than white iron. It will be noted that the improvement is almost as marked whether 0·05, 0·10, or 0·15 per cent. titanium is added, which apparently indicates that once deoxidation has been effected further improvement does not follow further addition. Hence in ordinary practice additions of 0·05 per cent. titanium will be sufficient.

BLAST FURNANCE PRACTICE.

Decreased Iron Content in Ore.—American ores are decidedly rich, a feature contributing in no small measure to the large make obtained per furnace; but for some time there have been indications that the metallic value of the ore was beginning to fall. The *Iron Trade Review*, in editorially discussing this question, makes excellent use of the annual statistics of the American Iron and Steel Association. These statistics include the quantity of limestone used in making pig iron. The

* American standard bar 1¼ diameter, tested on supports to 12 ins. apart.

total thus ascertained is, of course, not indicative of practice under given conditions, but the figures are none the less instructive. Coke and anthracite pig iron is separated from charcoal pig iron, a necessary distinction, since the amount of limestone used in making charcoal pig iron is very small. The quantity of limestone used per ton of pig iron in coke or anthracite furnaces has been as follows, no figures being available for the year 1899:—

LIMESTONE PER TON OF PIG IRON.

Year.	lbs.
1897	1002·3
1898	1020·4
1900	1222·9
1901	1202·7
1902	1207·7
1903	1207·3
1904	1128·0
1905	1144·1
1906	1192·8
1907	1247·8

The year 1897 marked a low point in prices, and only the richest ores were mined in that only the best could be marketed. Only the most favourably situated furnaces could operate, and the result was a low average limestone consumption. The average of 1,002 lbs. of limestone per ton of pig iron shown in 1897 was not the average of the country. Furnaces fed on Lake Superior ores used less and Southern furnaces more limestone. In 1907 the consumption represents nearly 1,250 lbs., but the proportion of the country's total pig-iron made in the South had decreased. Therefore, the Southern furnaces pulled up the average less in 1907 than they did in 1897, yet that average increased by one-fourth. It is evident that the limestone consumed by furnaces tributary to Lake Superior ores increased by considerably more than this. A small portion of the increase may be due to the use of coke carrying more ash, but the great bulk of the increase is due to the lower metallic value of the Lake Superior ores, leaving more material to be fluxed away. The improvements in furnace practice should, if anything, result in lessening the limestone consumption.

Published analyses of Lake Superior ores confirm this decrease. Thus the season 1898, giving chiefly average cargo analysis shown in 1897, shows that there were thirty-six ores listed from the Mesabi range. In the natural state these ores gave an average iron content of 56·36 per cent. The present analysis book lists seventy-four ores from the Mesabi range, and in their natural state their average iron content is 51·73 per cent., showing a decrease of 4·63 units in ten years.

These figures do not, of course, give absolute values, in that some of the choicest Mesabi ores are not listed, having passed to the control of steel interests which exclusively use them. None the less the known decrease in metallic value of certain of the ores coincident with the increase in limestone consumption is extremely suggestive to the student of American iron-producing conditions.

Gayley Dry Blast.—Following our notice of last month, we gather from a recent issue of the *Iron Trade Review* that the Gayley dry air blast is to be installed at nine additional Western furnaces. Of greater interest is the announcement that, following a thorough test with the Bessemer process at the South Works of the Illinois Steel Co., the United States Steel Corporation has authorised an expenditure for a permanent installation in connection with this Bessemer plant. In commenting on these new installations our contemporary states that there is apparently no limit to the adaptability of dry blast. In the blast furnace desiccated blast has been proved to lead to great economies through the establishment of uniformity in furnace operations. In the Bessemer process a more uniform and certain quality of steel is secured with greater economy.

STEEL: ITS PROPERTIES AND TREATMENT.

Some Features of the Steel Rail Situation.—Several papers bearing on the American rail situation were noted last month, but a reference of Dr. C. B. Dudley's presidential address* had to be held over. In this address Dr. Dudley reviews conditions leading up to the present "situation." Summarising this review, the first requirement is that of positive, definite information. In the discussions of the past two years opinions have been plentiful, but reliable information has been scarce. In this respect the steel makers have not been so deficient as the railroad engineers. The time is now opportune for genuine progress to be made. Whether the Bessemer process can be so modified and improved as to enable it to furnish rails that will be entirely satisfactory under the heavier wheel roads and denser traffic of to-day and the near future, or whether the basic open hearth will soon be the source from which steel for

* American Society for Testing Materials.

these rails will be furnished, are questions worthy of serious attention. Dr. Dudley's feeling is that if a small fraction of the time and money that has been spent in the past over the commercial development of the Bessemer process shall in the next few years be spent in obtaining sound ingots free from blow-holes, slag and manganese sulphide, and in overcoming or minimising segregation, the Bessemer process will last for many years to come.

Opportunities for Experiment.—The *Iron Trade Review* editorially draws attention to the value of "quiet times" as a means of improving products. Periods of lessened demand afford rare opportunities for manufacturers to better their positions in matters which cannot be attacked when the pressure for material is great. In particular reduced pressure gives opportunity for "weeding out." The present is an excellent time for steel producers to consider the question of improving the quality of the steel and for steel users to consider the question of improving the quality of their finished products. All available evidence points to the fact that there have been great changes in the quality of steel desired since the last period of general quietness, and these newer requirements should be studied whilst the opportunity offers, with a view to the bearing on future conditions.

STEAM VEHICLES IN RUSSIA.—H.M. consul at Kieff reports that motor cars are gradually coming into use, notwithstanding the want of passable roads in the country, and the rough "cobbly" paving in the towns. During 1907 a start was made in Kieff with cars plying for hire, and met with a fair measure of success. The import duty upon motor cars to seat two persons is £14 13s. 6d., and four or five persons £23 1s. 3d. In comparison with this it is interesting to note that motor vehicles or tractors, using petrol, kerosene, or alcohol as fuel, are charged a duty £1 0s. 10d. per cwt., and as this type of vehicle weighs from 4 to 6 tons, the duty amounts from £83 6s. 8d. to £120, which seriously handicaps the introduction of this class of vehicle for agricultural or commercial purposes. The ordinary steam traction engine, when imported with steam ploughs or threshing machines, pays a duty of 4s. 10d. per cwt., and, considering that petrol or kerosene tractors are available for ploughing, hauling and driving threshing machines, it is difficult to account for the high rate of duty charged upon them. Further, the Russian authorities, in putting obstacles in the way of the introduction of such tractors, overlook the fact of this class of engine being a source of revenue. Upon every pound of petrol or kerosene used there is an excise duty of 3s. 10d. per cwt. payable to the Crown, and therefore, on economic grounds, this class of tractor should be admitted under the same conditions and the same rate of duty as the steam tractor. There is an opening for this class of vehicle, and as British makers seem to be leading in this line, a combined effort should be made to have the question of duty looked into. The British-made powerful steam traction engine is far too heavy in weight for Russian requirements, and the lighter petrol type, judging from the reports of the working of one or two already introduced into the district, is considered as being the future motive power of the farm.

MACHINE TOOLS IN RUSSIA.—H.M. consul at Kieff states that the high duty of £1 7s. 6d. per cwt. chargeable upon machine tools has led a well-known German firm to adopt the following method of retaining its Russian trade. The heavy framing, lathe beds of cast iron (and, where necessary, with planed surfaces), are made in Russia, while the gearing shafts and other working parts requiring accuracy and skill in manufacture are imported from Germany. Formerly, when there was a considerable difference in the tariff charges upon machine parts and the complete machine, such a system was not possible, but under the present tariff the difference in rates charged is no obstacle to the aforementioned method.

British-made oil engines, for use either with kerosene or crude oil, are again finding an outlet in this district. The principal competitors are German and Swedish-made engines, and copies of Swedish and American engines made in Poland. The principal buyers are peasants, who use the engines for driving small flour mills.

ELECTRICAL MACHINERY IN SPAIN.—A consular report states that in the North of Spain buyers are so accustomed to deal with German and Swiss firms only in the electrical machinery trade, that it is difficult to make them believe that similar goods are manufactured in the United Kingdom, and the only practical way for British firms to secure any share of this trade is to combine to open branches in Spain carrying representative stocks and with a staff of technical experts who can speak Spanish. In the consul's opinion the electrical machinery and motor trades in Spain possess considerable potentialities, but British industry must adopt aggressive tactics to derive advantage from them.

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In this section is presented every month a list of the more important articles appearing in the Engineering Press and of the leading papers read before the various engineering institutions. Full names, addresses, and published prices of the publications indexed are printed each half-year in the volume index.

Civil Engineering.

Pneumatic Caissons. By T. K. Thomson. *Railway Gazette*, 21*st* and 28*th August*, 1908.

Anchorages for Blackwell's Island Bridge Cantilever Spans. *Engineering Record*, 8*th August*, 1908.

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The Wrought Compressive Member for Bridge Trusses. By H. E. Horton. *Journal Western Society Engineers*, *June*, 1908.

Minimum Rivet Pitch in Plate-Girder Flanges. *Engineering News*, 6*th August*, 1908.

Reinforced Concrete Bridges for Track Elevation on the Illinois Central R. R.: Failure Tests of Very Large Concrete Slabs. *Engineering News*, 6*th August*, 1908.

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Some Problems in Reinforced Concrete Engineering. *Engineering Record*, 22*nd August*, 1908.

A Formula for Calculating Flashboards for Dams, *Engineering Record*, 22*nd August*, 1908.

A Formula for Columns. By C. F. Marsh. Concrete and Constructional. *Engineer*, 8*th September*, 1908.

Glasgow Harbour Extensions. *Engineer*, 4*th September*, 1908.

The Prevention of Seepage from Ditches and Canals. *Engineering Record*, 29*th August*, 1908.

Heating and Ventilation.

Cooling and Ventilating the New York Subway. An abstract of Bion J. Arnold's report to the New York Public Service Commission of District No. 1 on the Subway Ventilation and Cooling Problem. *Electric Railway Journal*, 29*th August*, 1908.

Building.

Reinforced Concrete (*continued*). By G. C. Workman. *Practical Engineer*, 21*st August*, 1908.

Some Notes on Steel Constructional Work for Central Stations. *Electrical Review*, 8*th August*, 1908.

Materials and Measurements.

Testing Steel Tapes at the National Bureau of Standards. *Engineering News*, 13*th August*, 1908.

Tests on Bonding New Concrete to Old. *Engineering News*, 13th *August*, 1908.

Voids, Shrinkage and Weight of Crushed Stone. *Engineer*, 28*th August*, 1908.

The Influence of Fine Grinding on the Physical Properties of Portland Cement. *Engineering Record*, 15*th August*, 1908.

Cement: its Use and Abuse. *Journal Franklin Institute*, *August*, 1908.

A Test of Large Reinforced Concrete Beams. *Engineering Record*, 1*st August*, 1908.

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Boilers, Furnaces, and Fuels.

British Coal Dust Experiments. *The Colliery Guardian*, 28*th August*, 1908.

Modern Steamship Boiler Practice. *Electrical Review*, 4*th September*, 1908.

Engines and Motors.

Steam Turbine Construction (*continued*). By T. Franklin. *Mechanical World*, 28*th August*, 1908.

Recent Optical Indicators. *Practical Engineer*, 21*st August*, 1908.

Triple-Expansion Pumping Engines, Selby Waterworks. *Engineer*, 28*th August*, 1908.

Steam Turbine Blowers. *Engineer*, 14*th August*, 1908.

Steam Turbines in Power Stations. *Electrical Review*, 4*th September*, 1908.

Hydraulics and Compressed Air.

Pump Practice. By W. H. Booth. *Mechanical World*, 14*th August*, 1908.

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Shop Equipment and Practice.

Machine Shop Work (*continued*). By Jos. Horner. *Mechanical World*, 14*th* and 21*st August*, 1908.

The Designing of Machine Frames. *Machinery*, *August*, 1908.

A System of Supplying and Registering Loose Tackle and Cutting Tools in Engineering Machine Shops. By H. L. Smith. *Page's Weekly*, 28*th August*, 1908.

Miscellaneous Mechanical Subjects.

A Note on Condensation. By M. Leblanc. *Engineering*, 28*th August*, 1908.

The Mechanical Engineering of Collieries. By T. C. Futers (*continued*). *Colliery Guardian*, 21*st* and 28*th August*; 4*th September*, 1908.

Gear Cutting Machinery (*continued*). By. R. E. Flanders. *Machinery*, *August*, 1908.

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The Connecting Rods of Petrol Engines. By G. Stewart. *Motor Traction*, 22*nd* and 29*th August*, 1908.

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The Hampton Doctrine in Relation to Sewage Purification. By Dr. W. Owen Travis. *Engineering Record*, 15*th August*, 1908.

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Regina Bogenlampen-Fabrik, Köln-Sülz.—The new catalogue recently issued by this firm, comprising upwards of 100 pages, deals in a comprehensive manner with the electric lighting specialities manufactured by this company. Among these may be noted the new "Regina" arc lamps with a long carbon life, the "Helia" arc lamps for low current consumption, and the "Reginula" miniature lamp, giving a high illumination with a life of thirty hours. The latter are of capsular construction, similar to that adopted in the "Regina" series, and are unaffected by the weather. Detailed descriptions are also given of the new "Prima" flame arc lamps for double connection with the carbons in juxtaposition. A noteworthy feature of these lamps is the adequate protection afforded to the lamp-rods and the regulating mechanism by which a high durability of the lamps is guaranteed. Mention may also be made of the recently introduced projector lamps and searchlights for marine or land application, as well as special lamps for the production of blue prints, lithographic and photographic printing, for the preparation of line or half-tone illustrations, and studio lamps for the artificial lighting of photographic studios. There is also described an automatic blue print machine, the "Komplet," which prints and washes, dries and unrolls automatically the ferro-prussiate paper.

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